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ART. I.—*Geological and Miscellaneous Notice of the Province of Tarapaca;* by JOHN H. BLAKE—with a map.

TARAPACA, the southernmost province in Peru, is situated between latitude 19° and 21° 30′ S.; the Andes on the east, which separate it from Bolivia, and the Pacific Ocean on the west.

This province possesses many points of interest to the naturalist, and affords perhaps as interesting a field for research as any other portion of the continent of South America of the same extent. It forms a part of the great desert of Atacama; but though entirely destitute of vegetation, excepting in a few spots which are irrigated by water derived from the melting snow of the Cordilleras, it is much less desolate and lifeless, in its general aspect, than that portion to the south, which forms the western part of Bolivia and the northern part of Chili, where the oases are more widely separated, and throughout a large tract of country no living thing is to be found.

Two ranges of mountains and a plain, nearly level, extend north and south throughout the province; and between the Andes and the eastern chain of mountains lies an extensive plain inclined to the west. The surface of this latter plain is broken by numerous streams from the Cordilleras; it is formed of debris from the Andes, and is covered by huge angular masses of feldspar and trachyte, and numerous fragments of pumice and grains of sulphur. Among the neighboring mountains are seven or eight volcanoes, some of which occasionally emit a small volume

of sulphurous vapor. Parts of this plain afford support to coarse grass and bushes; one kind of Cactus grows rankly, attaining the height of eighteen or twenty feet, and a foot or more in diameter. It is collected by the inhabitants, and being split, serves for doors and rafters to their houses.

The range of mountains bounding this inclined plain on the west, and separating it from the great pampa, or plain, of Tamarugal, is composed of sandstone, with beds of gypsum, and is intersected by deep and abrupt ravines, some of which extend to the sea, while others terminate on the eastern border of the plain. The range of mountains on the western side of the pampa of Tamarugal is composed of feldspar-porphyry, resting on granite. The base at the west, in many places, is washed by the Pacific Ocean; but in parts of the coast, as at Iquique, a plain of several square leagues intervenes, composed of shells adhering together and of the same species as now exist in great numbers on the shore. The greater portion of these shells are partially decomposed, and may be easily crumbled to powder, while many of them are perfect and bear no marks of abrasion. Inland toward the mountains they form a compact uniform bed, scarcely a trace of the original shells being discernible, and as we approach the shore the forms become gradually more distinct till we meet with the living shells on the coast. Near the mountains the plain is covered with fine siliceous sand, the position of which is constantly shifting by the wind, forming hills of considerable elevation, and presenting a great variety of beautiful plains and curves.

The pampa of Tamarugal, lying between the two ranges of mountains before mentioned, is from three thousand to three thousand five hundred feet above the level of the sea. At the north it is bounded by a similar plain of greater elevation, and at the south by a deep and broad *quebrada*, through which runs an inconsiderable river, called the Loa. The surface presents clay, sand, gypsum, and common salt, mixed with nitrate and sulphate of soda. The three former substances, separately, cover large tracts of country, as do also the latter, united in various proportions, and nearly free from earthy matter. In some parts of the pampa, particularly at the south and east, the beds of clay are many miles in extent, and present a surface uniformly smooth and level, and so hard that when riding over it the hoofs of the mules make no impression. On the eastern side, further north,

the surface is sandy, and scattered over with numerous fragments of pumice, basalt, chalcedony, carnelian, and agate. Between Matilla and the mountain of Chalacollo, the soil is covered in several places with calcareous tufa for the space of an acre or more. Shrubs are here standing in the same position in which they grew, the smallest twigs remaining, presenting the singular spectacle of a once rank and luxuriant thicket converted into stone. Gypsum, more or less mixed with fragments of shells and marl, constitutes a large part of the surface of the pampa at the north. In the extreme northern part it presents a very remarkable appearance, being in flat rounded masses, slightly concave on the upper surface, from five to fifteen inches in diameter, and from one to two inches in thickness. They are compact and hard, and contain a few minute fragments of basalt. The same form also occurs in the beds of salt, which likewise constitute a large part of the northern section of the pampa, but in much larger and less regular masses; presenting on a grand scale the same appearance which is observed when evaporating saline solutions, where pellicles form and fall to the bottom of the vessel. These cakes of salt, many of them five or six feet in diameter and a foot thick, contain little insoluble matter; they lie piled one upon another to the depth of several feet, presenting a rough, white, and glistening surface, over which the traveller may ride all day without his horse's hoofs once touching the soil. Although more abundant at the north, these unmixed beds of salt are found in other parts of the province.

In the western part of the pampa, in latitude 19° 50′, at an elevation of about three thousand five hundred feet above the sea, and about two hundred feet above the adjoining plain, limestone, containing shells, rises from a bed consisting of pebbles and shells which are cemented together by salt, principally nitrate of soda. Part of the shells are decomposed, while others are perfect in form, and like those still found living on the rocks in the inlets of the sea. The same variety of limestone occurs on the opposite side of the mountains, near Molle, and is traversed, as is also the feldspar-porphyry of the neighboring mountains, by veins of the same salts which unite the shells and pebbles of the plain. Among the sandstone hills on the opposite side of the pampa, particularly in the vicinity of Pica, similar veins of anhydrous sulphate of soda occur. Many of them are a foot wide,

and can be traced for several hundred yards; they are compact, hard, and dry, and at a little distance resemble veins of quartz. Barely a trace of insoluble matter was found in specimens taken from different parts of several veins. Some of the cavities afford small rhombic crystals.

In the northern and eastern parts of the province are numerous quebradas, or abrupt ravines, commencing at the base of the Cordilleras and extending in a westerly direction; some of them intersect the pampa and both ranges of mountains, others terminate at its eastern border, dividing the eastern range only. Numerous similar ravines intersect the country situated between the Andes and the sea, both north and south of the province we are describing. A remarkable feature disclosed by them is a difference of level on the sides, which has evidently been occasioned by the upheaving of the one or the subsidence of the other.

These quebradas vary in depth from a few hundred feet to three thousand feet below the level of the plain, and in width from a hundred yards to five or six times that distance. The bottoms are covered with sand and pebbles, bowlders of porphyry, feldspar, and granite, and huge angular fragments of trachyte, sandstone, and gypsum. In various parts of these ravines, where the rock is exposed, both on the bottoms and on the sides, are deep scratches or grooves, running in the direction of the ravine. The sides present bold precipices. On those of the quebradas which terminate at the border of the pampa, near the mountain of Chalocolo, water lines are plainly discernible; and the crevices in the rock, at an elevation of several hundred feet above the plain, are filled with the same kind of clay which covers for miles this part of the pampa.

The quebradas are generally barren; but in some parts of that of Pisagua, *alfalfa* is raised in considerable quantity. In the eastern parts of those of Camarones, Chisa, Pisagua, and Tiliviche, are small streams which take their rise in the Andes; they are absorbed or evaporated before they reach the sea.

Beneath the surface of a part of the pampa, lies an extensive forest of large trees, all of which are more or less inclined to the southwest. They are, for the most part, of the Algarobo species. The wood is dark brown, inclining to red, and very brittle; it burns freely and with little smoke, although it contains a large

portion of resin. Parts of some of the trees have the appearance of having been charred. From latitude 20° I have traced this forest for nearly sixty miles in a southeast direction. About thirty miles further north, trees have also been discovered, and it appears not improbable, that the whole of this now barren plain was once a fertile and thickly wooded valley. In some places the branches of the trees are near the surface; and often, receding from these points in all directions, they are found more deeply buried, indicating an uneven surface of the valley in which they grew.

By sinking wells through the saline soil of the pampa, water has been found in some places at the depth of ten or twelve feet, while in other parts excavations have been made eight or ten times this depth without meeting with it. In general, after passing a few yards through marl, the wells terminate in a layer of coarse sand. On the western border of the pampa are several wells which have been sunk through trachyte, and brackish water obtained at a depth of from twenty to thirty five feet. In the neighborhood of Almonte, during my visit to that place, workmen were engaged in sinking a well, and had then attained the depth of one hundred and fifty feet without meeting with water. This well passed fifty feet through marl and clay, two feet through coarse sand, eighty feet through clay, ten feet through fine gravel, and terminated in a bed of coarse gravel and pebbles, mixed with large water-worn stones.

In the vicinity of Pica are two hot springs, one of which is 92° and the other 98° Fah. The water contains a small portion of carbonate of soda.

Among the hills which skirt the coast, and at their base on the western side of the pampa, are beds of *nitrate of soda*, which cover a tract of country not less than one hundred and fifty miles in extent. They are slightly elevated above the level of the plain, and covered by a light, dry, sandy marl, mixed with minute fragments of shells. This covering yields with a crackling noise to the pressure of the feet while walking over it, and thus affords an indication of the presence of nitrate of soda beneath, and is a common guide for those who are in search of it. Below this, and but a few inches from the surface, there is usually a layer of common salt, about a foot thick, possessing a coarse fibrous structure. Under this lies the nitrate of soda, resting on

marl impregnated with saline matter and mixed with fragments of shells.

This salt, technically termed *caliche*, varies in the quantity of nitrate of soda which it affords, from twenty to seventy five per cent. With it there is generally more or less insoluble matter, consisting of red marl and fragments of shells, in some beds amounting to nine per cent., but averaging not more than three per cent. It possesses a granular structure, arising from irregular rhombic crystals, which vary considerably in size in different localities. Some of the beds are exceedingly compact, and when wrought, require to be blasted with gunpowder; while others are easily broken with the aid of a pick and shovel. Cavities are occasionally found partly filled with crystals, regular in form and nearly pure. The color varies in different beds, and in different parts of the same bed. Some specimens possess the whiteness of refined loaf sugar; others are reddish brown, lemon yellow, and gray. Every variety is found in the same bed, but the compact white and yellow is most abundant between the quebrada of Tiliviche and the point called Molina. The composition of average specimens from the beds which are worked, as determined by Mr. A. A. Hayes, is as follows:

Nitrate of soda,	64.98
Sulphate of soda,	3.00
Chloride of sodium,	28.69
Iodic salts,	0.63
Shells and marl,	2.60
	99.90

We are indebted to the same gentleman for our knowledge of the presence of *iodate of soda* and chloro-iodate of magnesia in combination with this salt.*

In various parts of the western coast of South America, between 18° and 23° of south latitude, nitrate of soda is found impregnating the soil in connection with other saline matter, and in some instances forming a thin crust on the surface; but no where in extensive beds as in the province of Tarapaca, between 19° 30′ and 20° 45′ south latitude, and 69° 50′ and 70° 5′ west

* The 'mother' water, at some of the refineries on the pampa, are very rich in iodic salts; their presence was first observed by noticing the deep blue color produced by some crumbs of bread which had accidentally fallen into the vats.

longitude, although it has been frequently mentioned by travellers as abundant in other parts of the coast. This error has probably arisen from the general use of the term *sallitre*, which is applied alike to saltpetre and other salts.

The nitrate of soda of Tarapaca affords employment for a large part of the inhabitants of the province. In 1837 one hundred and fifty thousand quintals were shipped from the port of Iquique; of this, about two thirds went to England, and nearly one third to France. Its recent introduction as a manure will probably greatly increase the demand for it in foreign countries.

The process of refining, through which the crude salt passes before it is transported to the ports for exportation, is rude and simple. The operation is conducted generally by Indians, under the direction of a Spanish major-domo. Each *officina* or working place, consists of a few rude huts, the walls of which are constructed of cakes of salt, cemented together with the mixed marl and salt obtained from the kettles in use for refining, the roofs being formed of mats, supported by rafters of Cactus.

All the work of refining is conducted in the open air. The apparatus consists of a few copper kettles, of the capacity of fifty gallons each, set within walls formed of cakes of salt, and shallow oblong square vats for crystallizing. The salt, as blasted from the bed, which is always near to the officina, is carried in bags on the backs of laborers near to the kettles, where women and children are employed in breaking it into fragments of the size of hens' eggs. About two thirds of each kettle being filled with the broken salt, and water added, a strong fire is maintained until the water becomes saturated, when it is dipped into tubs to settle, and from thence transferred while hot to the crystallizers. The undissolved portion which remains, consisting principally of chloride of sodium and earthy matter, is thrown aside as worthless, although frequently not more than one half of the nitrate has been separated, the same relative proportion of crude salt being at all times used, without regard to its quality.

Aside from the want of economy displayed in the refining process, the affairs of the officinas are well conducted. Each branch of the operation, from the breaking the salt from the bed up to the time when it is placed on board vessels for exportation, is conducted by a distinct class of laborers, who receive for their work a fixed sum on each quintal of the refined salt produced.

The cost to the refiner for labor, for each one hundred and two pounds, is about five reals, or 62½ cents; for fuel, from two and a half to three reals; for powder and tools, about one real; and for transportation to the port, from five to six reals; making in all $1 87½, which is probably something more than the average cost of nitrate of soda, exclusive of bags for packing, and the expense of constructing, keeping in repair, and superintending the establishment.

Ores of silver, antimony, and copper, are found in the porphyritic hills on the coast—the two former near Iquique, in extensive veins, the latter in inconsiderable quantity near Pisagua and in the vicinity of Tanna. Copper also occurs at the extreme southern and eastern part of the province, in veins traversing feldspar. The ores are sulphurets, carbonates, and muriate. In the same range of hills, a little further south, and without the limits of the province of Tarapaca, this latter ore has been found in such quantity as to give rise to extensive workings. It is procured by the Indians, and sold, under the name of *arenilla*, as sand for letter writing.

The silver mines of Guantajay and Santa Rosa, near Iquique, were formerly extensively worked, and have yielded a large amount of silver; but of late years, owing to the increased expenses of mining and the poor quality of the ore obtained, most of the workings of the former, and many of those of the latter, have been abandoned. The mines, at the time when I visited them, did not yield ore containing in the mass more than 0.31 per cent. of silver; but they formerly yielded an abundance of rich ore, and have afforded some of the largest and purest masses of native silver which have been found. In 1758 and 1789, two are said to have been discovered here, the one weighing eight hundred and the other two hundred pounds. Native and horn silver are still often extracted, but it is from the antimonial silver ores that the principal profit is derived. A mixture of chloride, sulphuret and native silver, mixed with galena and accompanied by quartz, is found in some small veins. The matrix is generally carbonate of lime, and the veins vary in width from a size barely perceptible to more than a foot.

The observed temperature of the air at the bottom of the workings, in the mines of Santa Rosa, was 98° Fah. That of the air at the surface, at the same time, was 84° Fah.

There being neither water nor fuel in the part of the country where these mines are situated, the ores are transported on the backs of mules to Tirana, for the extraction of the silver which they contain. The process here made use of for that purpose is simple, and, compared with that adopted in many parts of the country, economical. The ore, after having been assorted and broken into coarse fragments, is ground to fine powder by means of a semicircular stone, resting on a flat horizontal bed, which is rocked back and forth on the ore by men stationed on each side. It is then mixed with calcined shells, salt, and mercury, and boiled with water in a copper pan for six or eight hours. When the amalgam formed is sufficiently rich in silver, it is pressed in skins, through the pores of which a part of the quicksilver passes, leaving a ball sufficiently solid to be removed to a stone bench, where it is laid on a grating and covered by an iron cone. This cone is then made secure, by luting at the base, and a fire kindled around it to expel the remainder of the quicksilver, which is collected in a dish of water placed beneath. The silver is left light and porous, and in this state is known as *pláta penia*. Purchasers before buying generally melt it and run it into bars, or expose it for a long time to a red heat in a furnace, in order to ensure the removal of all the quicksilver.

In the vicinity of Tarapaca, and in the quebrada called Cauisa, to the southward of Matilla, and in the mountains of Chalacollo and Chalacolito, are small veins of sulphuret of antimony, which afford traces of silver. Near Cocina is a small vein of galena, and in Cauisa there are extensive veins of iron ore.

In the towns of Tarapaca, Camina, Pica, Matilla and Tirana, the soil is cultivated; but in the remaining towns of Iquique, Pisagua, Mexellones, on the sea-board, and the mining settlements of Guantajay and Santa Rosa, the soil is barren, and the inhabitants are dependent upon distant places for their supplies of provisions and fresh water. The latter is frequently brought from Chili, and commands in Iquique the high price of six cents per gallon.

In those places where there are means for irrigating the soil, though nearly destitute of organic matter, it is extremely fertile and yields the various fruits common to tropical climes. In the town of Pica grape vines are cultivated, and the fruit affords a peculiar wine of excellent quality. The coast furnishes an abundant supply of *guano*, which is the only manure made use of.

Formerly this article was procured from a small island opposite to Iquique; but this source has now become nearly exhausted. On the coast, a few leagues to the southward, it is found in large quantity, and it is from this place that a large part of the guano used in the country is derived. A number of small vessels are constantly employed in the trade, and it has been estimated that a hundred thousand quintals are yearly sold in Peru.

The value of this substance as a manure was known to the Peruvians before the time of the Spanish conquest; it had been transported hundreds of miles for fertilizing the soil of distant places. It is still carried on the backs of mules over rough mountain paths, many leagues inland, and at a great expense, for the use of the agricultural districts of Peru and Bolivia.

Most of the inhabitants of the ports of Tarapaca, of which Iquique, containing about twelve hundred, is the principal, are engaged in the saltpetre trade, while those of the inland towns are dependent upon agriculture, mining, and the reduction of silver ores for their support.

The climate is highly salubrious, and many of the inhabitants live to a great age. The sky, generally deep rich blue, is sometimes diversified with a few light flocculent clouds, but it never rains. The air is clear and dry, and the heat of the sun's rays intense, yet, owing to the extreme dryness of the air, and the consequent rapid evaporation, if protected from the direct rays of the sun, one suffers but little from the heat of the climate.

Upon wetting the bulb of a thermometer with water, in the shade, the mercury has been observed to fall 18° of Fahrenheit's scale. Dead bodies dry without putrefying, and in all parts of the province where there is much travel, the dead bodies of mules and horses are seen, often thrown up in piles as landmarks for the traveller. In the church-yard at Iquique two bodies were left on the ground by their relatives, who were unable or unwilling to pay the fees required for their interment in consecrated ground. Six weeks after I saw them in the same spot; they had become dry and shrivelled, without emitting any disagreeable odor.*

* As a further indication of the extreme dryness of the climate, I may mention that while travelling further south, among the Andes of Atacama, I met with a vein of common salt, pure and beautifully transparent. Some of this salt I afterwards saw in an Indian village, ingeniously wrought into frames for prints—the lustre undiminished, and the salt and paper perfectly dry.

In the southern part of the province, the phenomenon called *mirage* is often witnessed. Beautiful lakes, sometimes dotted with islands and bordered with bushes, are presented, and so perfect is the illusion that I have more than once followed for miles these deceptive appearances, in the hope that they might prove real. Sometimes objects appear of enormous dimensions; and, by such as are familiar to us, a singular sensation is produced, as when meeting a traveller on horseback, who is distinctly seen and almost within speaking distance, yet appearing of gigantic size.

A sea and land breeze daily occurs, and the air from the mountains sweeping over the pampa often produces whirlwinds, which carry up columns of sand from eighty to one hundred feet high. During the night thin strata of air, coming from the mountains, and much colder than the surrounding atmosphere, are often felt, producing a sensation, on the exposed face or hand, not unlike that produced upon coming in contact with a cold rod of iron. Contraction of the muscles, attended with severe pain, is sometimes the consequence resulting from exposure to them, and the Indians, who term them *mal-ayres*, are careful to avoid them by covering themselves with their *ponchos* when sleeping in the open air.

The province of Tarapaca is not rich in remains of the ancient inhabitants; there are vestiges however of interest in several parts of the plain. On the summit of a very regularly formed conical hill, near Tanna, are two large circles, one within the other, formed of large blocks of stone, which were evidently carried there from a distant part of the valley beneath, and if without the aid of machinery, at an immense expenditure of labor. Similar circles of stones, like those erected by the ancient Celtæ, are not uncommon in Peru and Bolivia.

At the base of the hill are the remains of a few stone habitations, the walls of which have fallen and are nearly buried. Removing the sand from one of them, the floor was found to be composed of cement, smooth and hard. A few earthen vessels, and several flat and hemispherical stones, were discovered; the latter had probably been used for grinding corn.

A mile or two from this place is an ancient cemetery, where a large number of bodies have been interred. Unlike those near Arica and many other parts of Peru, these bodies have for the most part crumbled into dust. They are buried in a sitting

posture, with the arms crossing the breast; and are wrapped in cloths of woolen, some of which are fine and richly colored. As in the burial place near Arica, many of the skulls found here are elongated, full two thirds of the cerebral mass being behind the occipital foramen.

In the southern extremity of the pampa a single grave was discovered, distant from any remains of inhabitants, containing a body lying in a horizontal position, and dressed in skins of penguins neatly sewed together. At his side lay a bow and a quiver of arrows, the heads of which were formed of carnelian.

In various parts of the pampa are figures from twenty to thirty feet in size, formed in the sandy marl of the plain; the lines are from twelve to eighteen inches broad, and six or eight inches deep. The origin and meaning of these large hieroglyphics is unknown.

The most useful and extensive works of the ancient inhabitants which remain, are in the town of Pica, and consist of tunnels excavated through the sandstone of the inclined plain at the base of the mountains, for the purpose of obtaining water for irrigating the soil, and for which purpose they are still used by the Spanish inhabitants. These tunnels extend for a great distance, and when it is considered that they were formed without the aid of tools of iron, we must allow to the people who constructed them no small degree of skill, perseverance and energy.

ART. II.—*A New and Simple Method to find the Perpendicular Height of Mountains, Headlands, &c. above any given datum, from Barometrical and Thermometrical Observations;* by OLIVER BYRNE, Professor of Mathematics in the College for Civil Engineers, London.

[Communicated for this Journal.]

RULE.—Add the allowance found in Table I, for the difference of temperature taken by the attached thermometer, to the logarithm of that height of the barometer which corresponds to the least degree of the thermometer. Then to the logarithm of the difference of the logarithms of the heights of the barometer observed at the higher and lower stations, thus corrected, add the logarithm of the allowance found in Table II for the mean temperature of the detached thermometer, when increased by the

constant number .92102; this sum will be the logarithm of the required height in fathoms.

Obs.—The first four figures of the logarithms of the heights of the barometer, together with the indices, are to be counted whole numbers, and the numbers taken from Tables I and II must always have five places of decimals, though they need not always be used.

Tables I and II may be dispensed with, as 456789 answers to a degree of the attached thermometer in Table I, and 0024680 to a degree of the detached in Table II.

Table I.

Of the allowance for the difference of the temperatures of the attached thermometer.

Tens.	Units.	Tenths.				
1	0	4	5 6 7	8	9
2	0	9	1 3 5	7	8
3	1	3	7 0 3	6	7
4	1	8	2 7 1	5	6
5	2	2	8 3 9	4	5
6	2	7	4 0 7	3	4
7	3	1	9 7 5	2	3
8	3	6	5 4 3	1	2
9	4	1	1 1 1	0	1

Table II.

Of the allowances for the mean temperatures of the detached thermometers.

Hundreds.	Tens.	Units.	Tenths.				/
1	0	0	0	2 4	6	8	0
2	0	0	0	4 9	3	6	0
3	0	0	0	7 4	0	4	0
4	0	0	0	9 8	7	2	0
5	0	0	1	2 3	4	0	0
6	0	0	1	4 8	0	8	0
7	0	0	1	7 2	7	6	0
8	0	0	1	9 7	4	4	0
9	0	0	2	2 2	1	2	0

Previous to M. De Luc commencing his experiments on the barometer, it was taught that a mean between the two temperatures shown by the thermometer attached and the height of the mercury in the barometer at two different stations, was sufficient to determine the perpendicular distance of those stations. But De Luc found by repeated experiments that an additional or detached thermometer was likewise necessary, which has since been confirmed by General Roy, Sir G. Shuckburgh, and others. However, before making further remarks, we shall illustrate the rule and tables just given by practical examples.

Examples.

I. The heights of the barometer at the bottom and top of a hill are 29.862 and 26.137 inches; the attached thermometer at the bottom and top indicates 68° and 63°; also, the detached ther-

mometer at these stations gives 71° and 55° respectively. It is required to find the perpendicular height of the mountain.

Thermometer attached.		Thermometer detached.	
Lower station,	68°	Lower station,	71°
Higher "	63	Higher "	55
Difference,	5		2)126
		Mean,	63

Barometer at summit, where attach'd thermometer indicates least degree, } =26.137, log. 14172.557

From Table I, for 5 units, we have 2.28394

Log. corrected, 14174.84094

Barometer at the base =29.862, log. 14751.189

Take 14174.84094

Log. 576.34806 =2.7606848

Then, from Table II, for 6 tens, 14808

For 3 units, (making in all 63°,) 00740

Constant, 92102

Log. 1.07650 =0.0320140

Height in fathoms =620.4385, corresponding to log. 2.7926988

II. Wishing to know the perpendicular height of Chraughaim mountain in the county of Wicklow, and having two barometers and detached thermometers which for months before precisely agreed with each other in different states of the air; leaving an assistant on a level with the sea near Arklow, with directions to make accurate observations every fifteen minutes from 3 to 4 o'clock, (our watches being previously regulated,) I proceeded to the top of the mountain, and at the appointed hour commenced observations. The mean result of the five were as follows: the barometer stood at the summit 28.635, and at the base 30.609 inches; attached thermometer 61° and 65.5°, and detached thermometer 54.5° and 70°, respectively. It is required from these data to find the height of the eminence.

Thermometer attached.		Thermometer detached.	
Lower station,	65.5°	Lower station,	70°
Upper "	61	Upper "	54.5
Difference,	4.5		2)124.5
		Mean,	62.25

Barometer at summit, where attached thermometer is least,		=28.635, log.	14568.972
For 4 units, from Table I, we have			1.82715
For 5 tenths,			.22839
Log. corrected for temperature,			14571.02754
Barometer at the sea =30.609, log.		14858.491	
Subtract		14571.02754	
Log.		287.46346	=2.4585880
From Table II, we have,			
For 6 tens,	14808	making up for 62.25°.	
For 2 units,	00494		
For 2 tenths,	00049		
For 5 hundredths,	00012		
Constant,	92102		
Log.	1.07465		=0.0312671
Hence the height in fathoms = 308.926,			log. 2.4898551

It may be observed, that this experiment was repeated at different times, and consequently in various atmospheres, yet the result never varied two feet. We may therefore conclude that the highest summit of the Wicklow mountains is very nearly 1853 feet above the level of the sea.

The above rule will be found to give results as accurate as either that of General Roy or of Sir G. Shuckburgh, and can be applied with greater ease. The investigation of this rule will be given in the author's course of mathematics; the present performance is too limited to enter on such an enquiry.

General Roy makes the height in fathoms

$$=[10000l \mp .468d] \times [1+(f-32°).00245];$$

Sir G. Shuckburgh

$$=[10000l \mp .440d] \times [1+(f-32°).00243] \text{ fathoms.}$$

Where l = the difference of the logarithms of the heights of the barometer at the two stations; d = the difference of the degrees shown by Fahrenheit's thermometer, attached to the barometer; f = the mean of the two temperatures shown by the detached thermometer, exposed for a few minutes to the open air in the shade, at the two stations. The sign *minus* takes place when the attached thermometer is highest at the lower station, and the sign *plus* when it is lowest at that station. 10000 (log. M − log.

III) was the expression formerly given to find the altitude in fathoms; M, III being the heights of the mercury at the base and summit of any eminence.

This formula is very easily applied, and not far from the truth, when an allowance is made for the increase of temperature above 31°, for this is the degree of temperature to which the above formula is calculated.

As air expands very nearly $\frac{1}{4\,3\,3}$th part of its bulk for every degree of heat, and suffers the same contraction for every degree of cold, the following rule was usually given.

Rule.—Observe the height of the mercury at the bottom of the object to be measured, and again at the top, as also the degree of the thermometer at both these situations, and half the sum of these two last may be accounted the mean temperature. Then multiply the difference of the logarithms of the two heights of the barometer by 10000, and correct the result by adding or subtracting so many times its 435th part as the degrees of the mean temperature are more or less than 31°: the last number will be the altitude in fathoms.

Ex. III. If the heights of the barometer at the bottom and top of a hill are 29.37 and 26.59 inches respectively, and the mean temperature 26°, what is the height?

Log. 29.37 = 1.4679039
Log. 26.59 = 1.4247183

0.0431856

And $.0431856 \times 10000 = 431.856$.

Then $31° - 26° = 5°$ = the mean temperature below 31°.

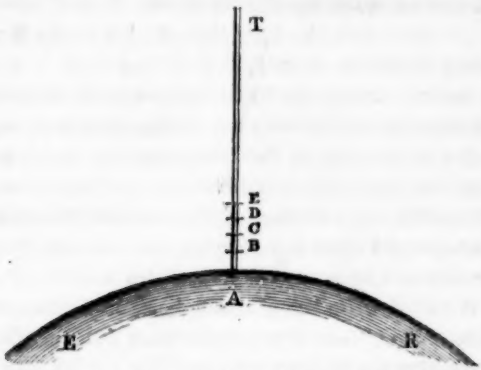
$\therefore 431.856 \times \frac{1}{4\,3\,3} \times 5° = 431.856 \times \frac{5}{4\,3\,3} = 4.964$, to be subtracted.

Height, $431.856 - 4.964 = 426.892$ fathoms.

We shall now investigate the last formula, to give an outline of the theory upon which this proposition is founded.

Let EAR represent part of the surface of the earth, and AT a column of the atmosphere. Conceive this column to be divided into a number of equal and infinitely small parts, as AB, BC, CD, &c., in each of which we may suppose the density to be uniform, because they are infinitely small. Now since the density of the air is always directly as the compressing force, therefore we have the density of the air in any of the portions AB, BC, &c. as the weight of the column of the atmosphere above that place; that is, if P represents generally the pressure, D the density of any

Mud-Head



place, P′ the pressure at any other place, and D′ its corresponding density, we shall have P : D :: P′ : D′; that is, the pressure is to the density in a constant ratio, and may be represented by n : 1;
$\therefore$ P : D :: P′ : D′ :: n : 1.

Consequently, $D=\frac{1}{n}P$,

$D'=\frac{1}{n}P'$,

$D''=\frac{1}{n}P''$, &c.

That is, the density at any place is equal to, or may be measured by the $\frac{1}{n}$th of the pressure of the column of the atmosphere above that place, or by the $\frac{1}{n}$th of the compressing force.

Hence, if we make P stand for the pressure at the surface A, and let each of the parts AB, BC, CD, &c. be equal 1, then will $\frac{1}{n}P$ represent the weight or pressure of the part AB, and

$$\therefore P-\tfrac{1}{n}P=\frac{n-1}{n}P=\text{the pressure at B,}$$

and $\frac{n-1}{n^2}P$=the density or weight of BC.

In the same way, $\frac{(n-1)^2}{n^2}P$=the pressure at C,

$\frac{(n-1)^3}{n^3}P$=the pressure at D, &c. &c.

So that the pressure, and consequently the density, will decrease in a geometrical progression, as the altitudes increase in an arithmetical progression.

Calling the density at the surface d^n, and the several altitudes 1, 2, 3, 4, &c., we shall have the following corresponding series.

Altitudes, 0, 1, 2, 3, 4, 5, &c.

Corresponding densities, d^n, d^{n-1}, d^{n-2}, d^{n-3}, d^{n-4}, d^{n-5}, &c.

Dividing the latter series by d^n, we have

Altitudes, 0, 1, 2, 3, 4, 5, &c.

Corresponding densities, $1, d^{-1}, d^{-2}, d^{-3}, d^{-4}, d^{-5}$, &c.

This is strictly analogous to the property of logarithms. In fact, the several altitudes form a peculiar system of logarithms, of which the reciprocals of the corresponding densities are the natural numbers; from this circumstance they have been denominated atmospheric logarithms. From a similar circumstance, the Napierian are termed hyperbolic logarithms, because they express the areas contained between the asymptote and curve of an hyperbola. We shall write these atmospheric logarithms with large letters—thus, "Log."—to distinguish them from the Briggean or common logarithms, which are written "$\log._{10}$" or simply "log." and also from the hyperbolic, which are denoted by "n log."

Let a, A represent any two altitudes, and d, D their corresponding densities.

Then will $A = -\text{Log. } D$,

and $a = -\text{Log. } d$;

$$\therefore A - a = \text{Log. } d - \text{Log. } D = \text{Log. } \frac{d}{D}.$$

Now it is a well known property in logarithms, that by assuming different values for the base, there will be as many different systems of logarithms; and it is equally well known, that in all the various systems of logarithms, the logarithms of the same numbers can be converted from one system to another, by a constant multiplier or *modulus*.

The object of our present enquiry is to determine a constant multiplier that shall convert the common logarithm of a number into the atmospheric logarithm of the same number. To accomplish this, let

$$\text{Log. } \frac{d}{D} = x \log. \frac{d}{D},$$

$$\therefore A - a = x \log. \frac{d}{D}.$$

Then making $a = 0$, or which is the same, if we suppose d to represent the density of the atmosphere at the surface of the earth, we shall have

$$A = x \log. \frac{d}{D}.$$

In order to find x, let us take the height of a homogeneous atmosphere, when the temperature shown by the thermometer is

31°, and the height of the barometer 29½ inches, at 26057 feet; then the density at the surface, and one foot above it, will be

$$a=0;\ d=26057.$$
$$\mathrm{A}=1;\ \mathrm{D}=26056.$$

That is, the pressure at the surface will be equal to a column of air of uniform density of 26057 feet high; and consequently, one foot above the surface =26056 feet high, or a foot less.

Since the densities are as the pressures, we have

$$\mathrm{A}-a=1=x\log.\frac{26057}{26056};$$

and making $26057=n$,

we have $1=x\log.\frac{n}{n-1}$.

But $\log.\frac{n}{n-1}=\mathrm{M}\left(\frac{1}{n}+\frac{1}{2n^2}+\frac{1}{3n^3}+\frac{1}{4n^4}+\&c.\right)$

And when, as in the present case, n is a large number, all the terms but the first may be neglected as unimportant; also since M=.43429448, the modulus of decimal or common logarithms,

$$\therefore 1=x\times\frac{.43429448}{26057};$$

whence $x=\frac{26057}{.43429448}=60000$, very nearly.

The above formula is reduced to

$$\mathrm{A}=60000\log.\frac{d}{\mathrm{D}}\text{ feet};$$

or putting m and M the height of the mercury at the earth's surface and at the altitude A, then the fraction

$$\frac{d}{\mathrm{D}}=\frac{m}{\mathrm{M}}.$$

Also since six feet are equal to one fathom, the simple multiplier 60000 for feet becomes 10000 for fathoms, which is more convenient.

Hence instead of $\mathrm{A}=60000\log.\frac{d}{\mathrm{D}}$ feet,

we have $\mathrm{A}=10000\log.\frac{m}{\mathrm{M}}$ fathoms,

which is the formula formerly used in measuring altitudes by the barometer.

With respect to the height taken for the homogeneous column of air different writers vary, but this difference does not affect

the ultimate result. It is well established, that the height of an homogeneous atmosphere whose density would be equal to that of the air at the earth's surface, and weight the same as that of the real atmosphere when compared with a column of mercury or any other fluid of the same weight, the heights will be reciprocally as the specific gravities of the air and mercury or other fluid. So that, if we take the specific gravity of the air at the earth's surface at $1\frac{1}{5}$ when compared with distilled water at 1000, and that of mercury at 14000, also the column of mercury in the barometer $=29\frac{1}{2}$ inches, we have

$1\frac{1}{5} : 14000 :: 29\frac{1}{2} : 344166$ inches $= 28680.5$ feet $= 5.43$ miles.

But the specific gravity of fluids varies as their temperatures vary. It has been found by various experiments, that when the mercury in the barometer stands at 30 inches, and the thermometer at 55°, the specific gravity of air, water and mercury are nearly as $1\frac{1}{5}$, 1000 and 13600. Hence,

$1\frac{1}{5} : 13600 :: 30 : 340000$ inches $= 28333\frac{1}{3}$ feet $= 5.366$ miles,

the height of a homogeneous atmosphere. Again, taking the specific gravity of the air at the earth's surface at $1\frac{2}{5}$, which some affirm, and the barometer at $29\frac{1}{2}$ inches, it will be

$1\frac{2}{5} : 13600 :: 29\frac{1}{2} : 328255$ inches $= 27358$ feet $= 5.1814$ miles.

Hence, generally, we may assume if the air was of the same density at all altitudes as at the earth's surface, its height would be between five and six miles. But it matters not what degree of temperature we assume, for we can always accommodate the result to any other temperature, as before observed, by augmenting or diminishing the result by the $\frac{1}{4\,3\,3}$th part for every degree above or below 31°.

It may further be observed, that the common barometer is the best and most to be depended on; for many which are said to be improved, have only the recommendation of deviating from it in simplicity. It appears from accurate observations, that mercury stands higher in tubes of a larger than in those of a narrower bore; and therefore, when observations are made with different barometers, attention should be paid to the difference of their diameters. In order to prevent the effects of the attraction of cohesion, the bore of the tube should not be less than one fourth of an inch; but one third of an inch would be better.

We cannot conclude this problem better than by giving a remark of Prof. Dalby's, whose practice in trigonometrical surveying was extensive.

"In determining altitudes by the barometer, it is best to make the observations at the upper and lower stations at one and the same time as nearly as possible; but great care must be taken that the two barometers and also the thermometers are alike; that is, they should precisely agree when together in all states of the air. It is also necessary that the specific gravity of the mercury be well ascertained, because it is not equally pure in all barometers, which is the principal reason why different results are frequently obtained from observations made with different barometers at the same stations. Other circumstances however, not generally known, may contribute to such disagreement."

Thus Mr. Ramsden proved by experiment, that the quicksilver in barometer tubes made of different sorts of glass, will be sustained at different heights.*

ART. III.—*Description of Ancient Remains, Animal Mounds, and Embankments, principally in the counties of Grant, Iowa, and Richland, in Wisconsin Territory;* by S. TAYLOR—with four plates.

FEW subjects have of late years more engaged the minds of scientific antiquaries, than the mounds in the valley of the Mississippi. It is in reality one fraught with much interest, and from which the veil of obscurity has never yet been drawn; and unless future investigations may illustrate facts which will aid the curious in tracing these antiquities to their origin, it is feared that the mystery will ever remain unsolved.

In the Holy Scriptures we find but a single passage wherein works in the form of mounds are spoken of, which passage instructed the wandering Israelites to establish mounds, in order to guide them on their return to the land of their nativity. The words referred to may be found in the twenty first verse of the thirty first chapter of Jeremiah, which are as follows: "Set thee up way-marks, make thee high heaps: set thy heart toward the high-way, *even* the way *which* thou wentest: turn again, O virgin of Israel, turn again to these thy cities." This passage, it would seem, strengthens the conjecture that the aborigines of

* It is usual at present, in accurate meteorological records, to note the difference of height between barometers of flint and hard glass. See the tables of the Royal Society.

America are descendants from the Israelites, and probably from the lost ten tribes. Historians also inform us that tumuli, similar to those found in this region, were constructed by some nations of antiquity, among whom were the Scythians and Tartars. Could we be led to believe that North America was originally peopled by these wandering hordes, we might safely conjecture the origin of the myriads of various formed monuments of antiquity throughout the "great west." One matter, however, at the threshold of investigation, casts an almost insuperable difficulty in our way; and this is, the fact of the non-existence of tradition among the present generation of the Indian race, by which we can have the least hope of unraveling the mystery. This matter I have made a subject of inquiry whenever meeting with an intelligent, communicative Indian, and I have found that the various tribes which inhabit this section of country, express total ignorance on the subject of the origin of the mounds; some however are impressed with a belief, founded upon their superstitious notions, that those in the form of animals were constructed by the "great Manitou"—that they are indicative of plentiful supplies of game in the world of spirits; they are, therefore, looked upon with reverence, and are seldom molested by them. Tribes and even bands differ in their conjectures with regard to them. In conversing with an intelligent French gentleman, who, as a trader, resided near the Pembina (Selkirk) settlements on the Red River of the north, upon the subject of the antiquities of that region, he related a circumstance, which, it would seem, throws a glimmering light upon the origin of one class of these ancient works. After the termination of a battle between the Chippeways and Sioux Indians, (in which in self-defence it became necessary for him to participate,) the women and children of the former, who were the victorious party, in celebrating the achievement, created a mound, from the adjacent surface, about five feet in height, and in diameter eight or ten feet, upon the summit of which a pole ten or twelve feet in length was planted, and to this pole tufts of grass, indicating the number of scalps and other trophies achieved, were tied; around this mound, the warriors, with their usual ceremonies, indulged in mirth and exultations over the scalps of their ill-fated foes.

In this territory we find these works of antiquity in the dense forest, giving nurture to the largest trees, which measure in some instances three feet in diameter, and are frequently based upon the

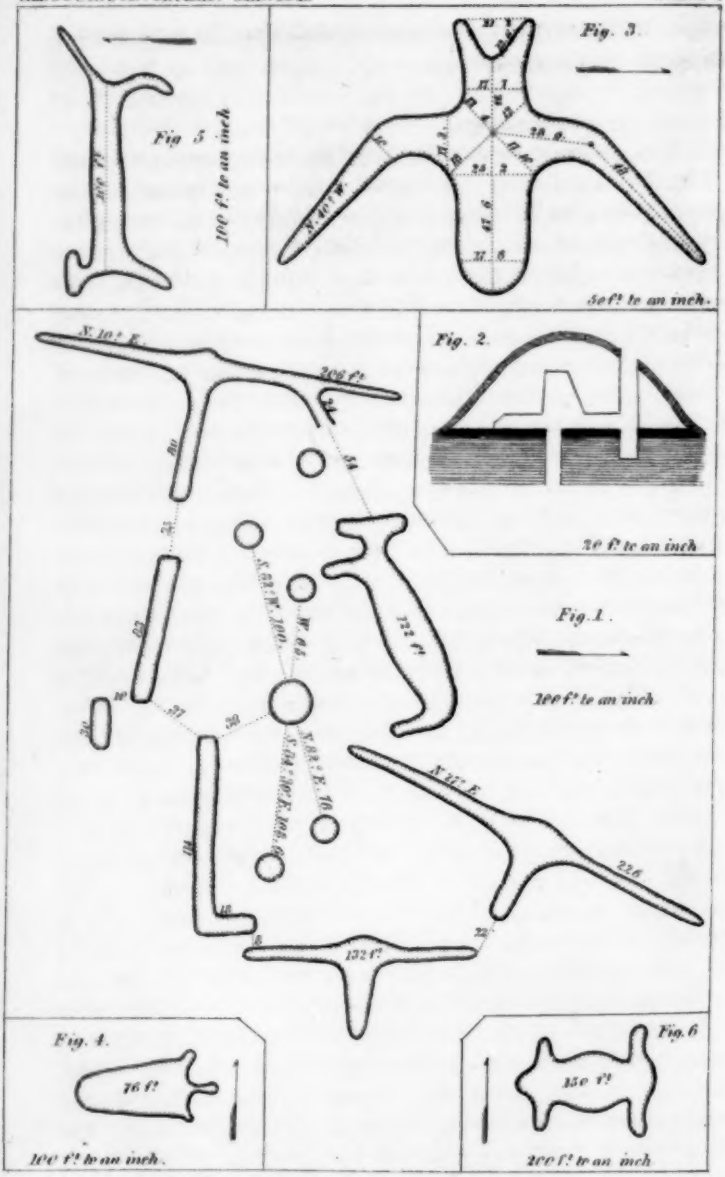
summit of some of these mounds, while upon others their branchless trunks lie prostrate and decaying, impressing upon the mind of the observer the vanity of the shadows which the people of an age long gone by had pursued. We also find them in the sparsely timbered regions, as well as upon the undulating prairie plains, principally in the vicinity of large water-courses, above the influence of high freshets or inundations. It is a remarkable fact, that they are seldom found upon hilly or upon sterile lands. They are also, so far as has come under my observation, confined to certain limits, seemingly in the form of the letter T, beginning at Prairie du Chien, on the Mississippi River, and extending eastwardly, embracing the remains along the Wisconsin River, Blue Mounds, and the Four Lakes, and continuing as far as Rock River, to where the top line of the T, extending from Green Bay, by the way of Fox and Rock Rivers, to the Grand de Tour, in Illinois, touches the upright part of the letter; these strips of country are in width in proportion to their length, covering the ancient works at Butte des Morts, Aztalan, Grand de Tour, and all intervening points. There are, however, many isolated groups in other parts of this region; for instance, those northwest of and in the vicinity of Galena, Illinois. Although their general position seems to be as curiously planned as the structures themselves individually are, it can be readily accounted for from the fact, as before stated, that they generally exist in the vicinity of large water-courses and lakes; and the main streams of the Fox River, of Green Bay, of Rock and Wisconsin Rivers, have their courses in the shape of the letter above mentioned.

May not those works which are found in the forests, have been constructed when the lands upon which they exist were yet prairie? That this region was once wholly submerged, as the sedimentary formations amply demonstrate, admits not of a doubt; subsequent to the subsidence of the waters then, and anterior to the occupancy of these lands by the primitive wanderers who constructed these works, some period must have elapsed; could the era when the waters subsided be traced by the fossils existing in the rock, the era of these ancient remains might be, possibly, conjectured.* We are all agreed, however, upon their re-

* It cannot be supposed that our correspondent would unite two periods so remote as the geological eras of the fossiliferous rocks of Wisconsin and the sedimentary deposits which cover them, both of which must be considered as very long anterior to the appearance of man on our planet.—Eds.

mote antiquity, although they seem to us, at this time, not more ancient than they appeared to others in this region a century and a half ago.

Many who have written upon this subject, view these antiquities collectively as cemeteries; they suppose that when a location for sepulchral purposes was selected, the grounds in the vicinity were held sacred; that in constructing these tumuli, the material was brought from a distance, or collected in such a manner as to exhibit no indications of the adjacent soil having been removed; and as from the nature of the structure a constant disintegration took place, posterity were enjoined, as they wandered to and fro, to add earth to the heap;—and they believe that by such means these monuments gradually rose into existence. It is very evident that these works *were* heaped up, and by a race that has long since passed away; as to the material of which they are constructed having been brought from a distance, we have no other testimony than conjecture. From the excavations around and in the vicinity of many of them, more especially those in the form of the cross, I am persuaded that the material of which they are composed was obtained from the ground adjacent to them; while in the vicinity of those of other forms, the surface does not appear as though the earth of which they were constructed had been taken therefrom; so that the conjecture appears plausible, that some of these works were heaped up with accumulated material brought from a distance: it is upon dark and mysterious conjecture alone, however, that we ground our opinions. It is true that many of these works are now used by the Indians as burial places; that when they are selected for this purpose, the corpse, after the manner of some tribes, is inhumed in a sitting posture, enveloped in a blanket, accompanied with all its paraphernalia, and as a protection from the ravages of wolves or other rapacious animals, the grave is enclosed with pickets meeting over the centre; (for sketch of Indian tumulus, see Plate VI, fig. 2.) Within the enclosure may be seen at times a supply of tobacco and some weapon of defence, and in some instances an ear of corn or other provisions are placed upon the grave! At the death of a distinguished chief or brave, as a token of the esteem in which he was held, at one end of the grave a post is planted, to which a white flag is appended; upon this post, stripped of its bark, the survivors



Ancient Remains, Animal Mounds, &c.

to face Page 25

inscribe, with red paint, the valor or worth of the deceased, in rude hieroglyphic characters.

Plate V.

Fig. 1 represents a group of earth-works, which I have presumed to designate as a *citadel;* it is situated upon section two, township eight, near the north bank of the Wisconsin River, one and a half miles west of the fourth principal meridian, in the county of Richland. The citadel is singularly planned; the walls comprise embankments of various forms, so arranged as to leave several openings or sally ports, guarded, however, by mounds in the interior; it occupies a prominent level space of about half an acre, the ground to the north, south, and westward, without the embankments for some yards, having a gradual descent, and to the eastward spreading into a beautiful plateau, which gives to the whole structure the imposing appearance of having been constructed as a place of refuge; upon the plateau, as well as to the southward, are numerous other embankments of various forms, the dimensions of some of which, they being disconnected, I did not take; to the westward, within four hundred yards, viewing them from the large or central mound in the citadel, may be seen at least a hundred similar to those forming the outlines of the citadel. The elevation of these embankments, generally, is no more than thirty inches, and of the lesser mounds twenty inches, while the altitude of the large mound, overlooking the whole group, is ten feet. The original sod, upon which the *walls* of the citadel rest, as well as the surface of the ground adjacent to many of them, does not appear as though it was used in constructing them. I made no excavations in these embankments, but from an examination of the central mound in the citadel, I have been led to the conclusion that they are similarly constructed. I am not borne out in this conclusion with regard to the construction of all the outer walls of the citadel; around those forming the east and northeast sides, excavations, from whence earth had been removed, are plainly indicated, so that the material, of which this portion of the work is constructed, was evidently obtained immediately adjacent. Notwithstanding the rank growth of vegetation upon all these works, and their having, in all probability, mouldered down from the original height

to their present level, the angles and terminations are quite visible; near the northwest angle of the citadel, however, part of the embankment seems to have been destroyed, although it can be traced some distance toward the head of the animal, represented as enclosing the northern side of the citadel.

Fig. 2 is a section of the mound above referred to, showing the manner in which it was examined, as well as the connection of the superincumbent stratum of sod, with that of the original upon which the mound is based. In the examination of this mound, as well as in the measurement of some of the embankments bounding the north side of the citadel, I am indebted to the kindly assistance of James T. Hodge, Esq. of Plymouth, Mass., a gentleman favorably known as having been a member of the geological corps of the states of Maine and Pennsylvania. Mr. H. also visited with me many of the figures noticed in this article. In order effectually to examine the construction of this mound, a shaft, about midway between top and bottom, of sufficient dimensions to remove the earth conveniently, was sunk; in sinking to the depth of eight feet, we reached the original sod, which here assumes a different character from that coating the mound and the adjacent surface, being a hard and compact substance, denominated "hard-pan," caused, no doubt, from the pressure of the immense weight of earth upon it for centuries. The superincumbent mass, being a bed of ferruginous sand, having no appearance of stratification, and being free from admixture, the presumption is, that this mound was *not* constructed, as suggested, by small contributions, but was heaped up in the progress of construction to its completion without intermission. Continuing the shaft through the original sod, (which measures here six inches in thickness,) three feet farther, we found the substratum to be composed of alternating layers of ferruginous earth and sand. Having now sunk the shaft eleven feet from the surface, to remove the earth without a windlass became too laborious; we then, commencing above the hard-pan, penetrated the mound westwardly fourteen feet, being some distance beyond the centre, conveying the excavated earth through the shaft to the surface. Directly under the centre of the mound, in the drift, we sunk another shaft five feet in depth; the substratum here compares with that in the first shaft. Having now found much difficulty in removing the excavated earth, our drift being partially filled, we con-

cluded, before we abandoned the examination, to oust a badger, which we had reason to believe had burrowed into the top of the mound, such places being favorable resorts for these animals. We found but little difficulty here in excavating the earth; it came down in masses, completely filling the last shaft, as well as the drift, leaving for us but a small aperture to make our egress. Having now thoroughly and satisfactorily examined the structure of the mound, which required the labor of two days, finding nothing of a curious nature in it, and feeling our position under this mass of arenaceous earth, completely hollowed out as it was, rather perilous, we made our way to the surface, which we reached in safety.

Fig. 3 I have designated the *horned bird* in my field-book, in order to distinguish it from others; its location is upon the east bank of Blue River, upon section sixteen, in township eight, of range one, west, in the county of Grant, where an extensive group of many hundreds of various forms may be seen. The numerical figures upon the drawing indicate its dimensions; the elevation of the figure at the breast is three feet, gradually diminishing toward the extremities of the wings, horns, and trunk, until they all, in a measure, become blended with the general surface. Bearings, east and west, with the head to the westward.

Fig. 4 approaches nearer to the form of the "turtle," so frequently spoken of by writers on the antiquities of the west, than any of the earth-works which have come under my notice in this region. Having searched in vain for a figure which, with a little stretch of the imagination, might be construed into the form of a tortoise, I have concluded that figures such as the one here represented, may have been by others styled the *turtle*. Figures of this description are by no means scarce in this region; the location of this one is within two hundred yards northwestwardly of the citadel represented in fig. 1; its length from the tip of the nose to its posterior extremity is seventy six feet, where in width it is eighteen feet, and over the projections representing claws, it is thirty seven feet; the greatest elevation near the junction of the neck is thirty inches, and at the narrow end fifteen inches; while the head, neck, and claws, are only nine inches. The whole figure, having a permanent coat of sward upon it, has retained its original shape in great perfection. Bearings, east and west, the head to the eastward.

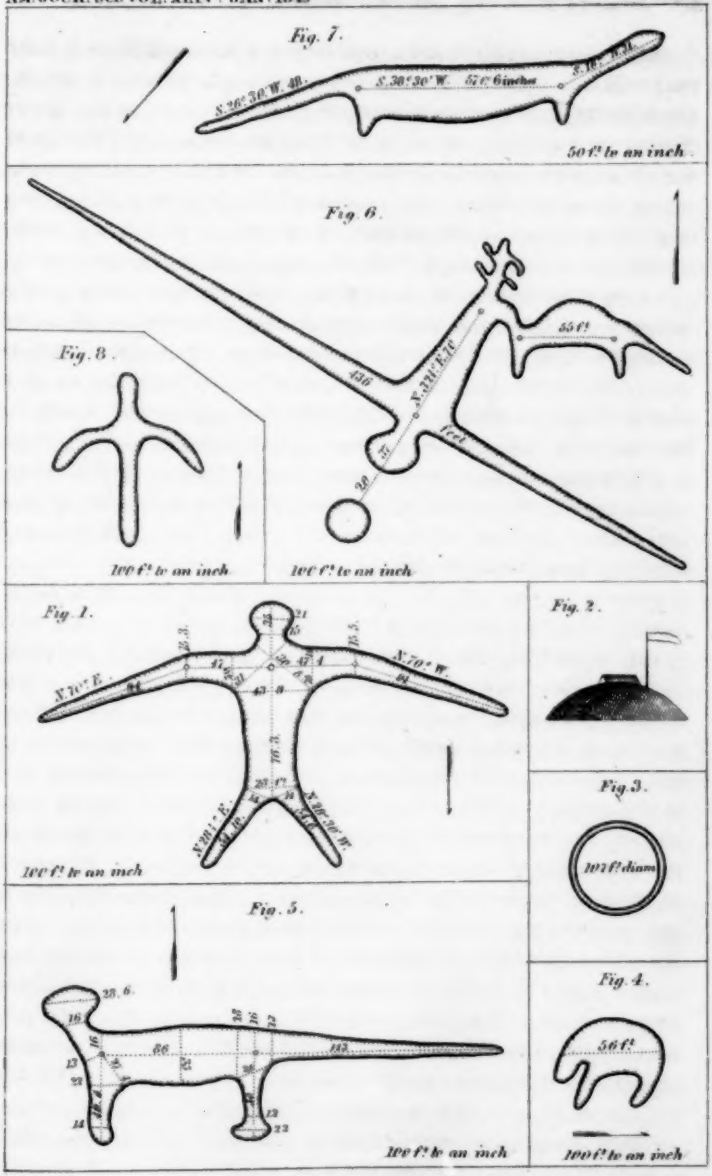
Fig. 5 seems to have been intended as a representation of some fleet animal, perhaps the deer or the elk; the antlers, however, seem shortened; the head, neck, and tail, are erect, as though in the act of running. This figure is situated within a few feet of the river, and to the southward of the *citadel;* its elevation is about eighteen inches, the length of the body is one hundred feet, and averages in width twelve feet. Bearings, east and west; the head to the westward, and the legs projecting southward.

Fig. 6 is an embankment, situated upon the north bank of the Wisconsin River, and *east* of the fourth principal meridian; it resembles very closely the form of the frog, for which animal it was probably constructed; there are others adjacent to it, of a similar shape, as well as several in the form of a cross, mammillary mounds, and parallelograms. The dimensions of this huge figure, being in length one hundred and fifty feet, may be ascertained by reference to the scale upon which it is drawn; its elevation above the general surface is three feet. Bearings, east and west, the head to the westward.

Plate VI.

Fig. 1. Among the various animal-shaped works of antiquity, in this region, those in the form of the human species are numerous. This figure forms one of an extensive group of these works, of various shapes, situated upon section thirty five, in township nine of range one, west of the fourth meridian, and in the margin of the forest, extending into it, and having large trees growing upon it.* It is truly a giant, and measures, from the extremity of one arm over the breast to that of the other, as will be seen by the measurements noted upon the figure, two hundred and seventy nine feet and eight inches, and from the top of the head to the end of the trunk, one hundred and eleven feet and three inches; over the hips twenty eight feet; its legs in length are fifty four feet and ten inches; the shoulders, head, and breast, are elevated four feet above the adjacent surface; from thence to the extremities of the limbs the

* The arrow accompanying this, as well as all other figures in the drawings, indicates the cardinal points. In copying many of these figures upon the crowded plates, in order to place them in a more favorable position, I have deviated from the usual mode of drawing; this will account for what might perhaps be otherwise looked upon as an error.



Ancient Remains, Animal Mounds, &c.

to face Page 28.

elevation gradually diminishes to one foot. Bearings, north and south, the head to the southward. In the centre of the breast of this figure, there is quite a depression. Indians frequently, after gathering their crops, dig pits in the earth for the purpose of securing their provisions and merchandise from the frosts of winter and depredations of the enemy; these pits, in the language of the French *voyageurs*, are technically called *caches*. The depression in this figure may have been occasioned by an excavation for this purpose. In a group of earth-works about a mile to the northwest of this one, another figure in the human shape, of like magnitude, may be seen; and of the same group, a very large mound forms a part. This mound, in circumference at its base, measures two hundred feet, and in height sixteen feet. The human figures, generally, do not seem to be as well proportioned as those in the form of quadrupeds; it is however strange that they should have been constructed upon such a gigantic scale.

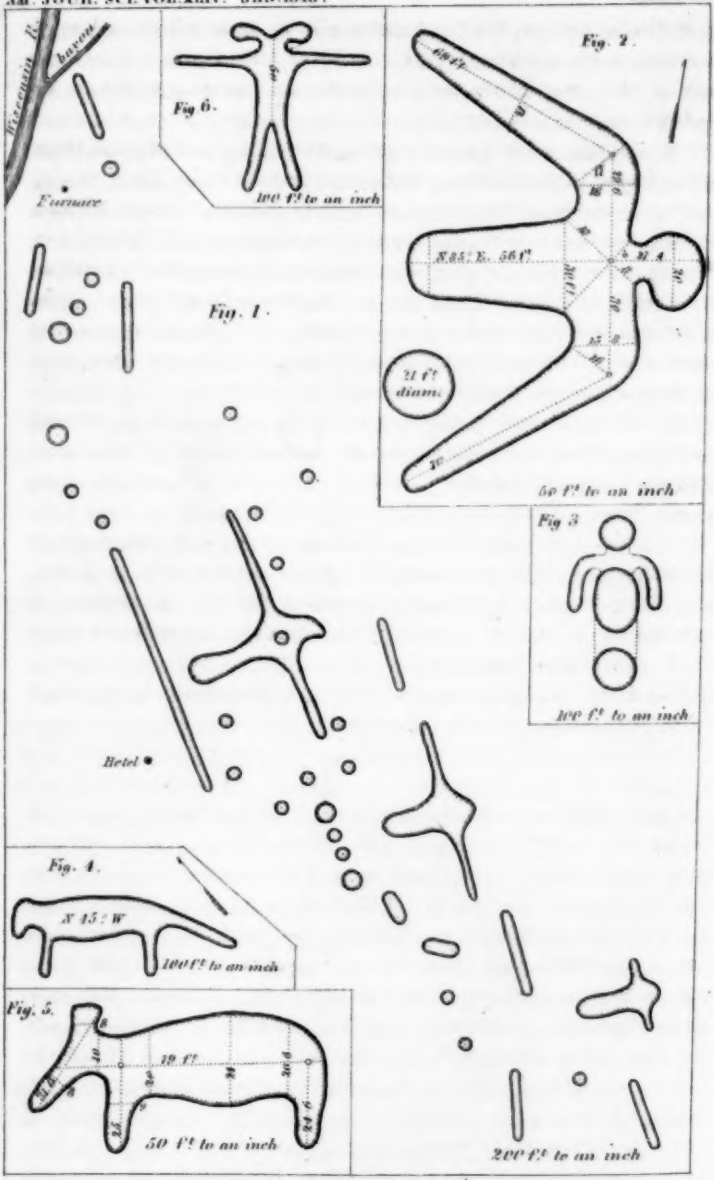
Fig. 3 represents a complete circle, situated two or three hundred yards to the northeast of fig. 1. In wandering through the forest in quest of strange figures, I accidentally discovered this circle; I have since searched for it for the purpose of ascertaining by excavation, whether it may not have resulted from the accumulation of ashes proceeding from circular fires, which Indians on some occasions make, but I was unable again to find it. The embankment of this figure, which I measured on the spot, is only ten inches in height, and in width six feet; the diameter of the circle is one hundred and seven feet.

Fig. 4 was originally an embankment, approaching the form of a bear, but at this time it is partially destroyed by a road passing over it. I fortunately took its dimensions when it was yet perfect; its whole length from forehead to rump was fifty six feet, which was, in comparison to others, of small dimensions; the elevation was only about twenty inches; it may be seen within a hundred yards of my cabin, one mile and a half north of the river in Richland county. Bearings, north and south; head to the southward, and the legs projecting eastward.

Fig. 5 is one of a group of three, closely resembling each other, in the western part of the village of Muscoda, in the county of Grant; its length from the front part of the head to the end of the tail, is two hundred and sixty four feet; the numerical

figures upon this one, as well as upon *all* others, indicate the dimensions of the structure, from actual survey and measurement. Its elevation is about thirty inches around the body, while the limbs at their extremities are diminished to a few inches. It lies within a few feet of the Wisconsin River, in an east and west direction, the head to the westward; and the legs of this one, as well as those of its companions, are projecting southward.

Fig. 6. The site of this singular shaped mound is upon an eminence, on section twenty seven, within a mile to the northwestward of the Eagle Mills, in the county of Richland. The northeastern part of the figure, especially from the neck eastward, approaches nearer the form of a buffalo, than any of these works which I have examined, having quite a protuberance, resembling the "hump" upon the back of that animal; the head, if I may so term it, is blended with what I conceive to be the trunk of the human figure, to which, projecting northward, are appended what were perhaps intended to represent horns. I must admit, that however much we may feel inclined, in viewing these antiquities, to let our imaginations lead us into erroneous ideas respecting them, I could not, at first sight, persuade myself that these appendages were really intended to represent horns; although after a second and third examination, I feel justified, from their intimate connection with the remainder of the structure, in designating them as such, notwithstanding they may, possibly, have been caused by the uprooting of trees. The elevation of the body of the part resembling the *buffalo*, near the hump at the widest point, is three feet; the legs, tail, neck, and horns, diminish as they recede to their extremities, to one foot. The southwestern part of the structure represents the trunk, head, and outstretched arms, of what may be termed the human figure, the arm extending northwestwardly being much the longest; the head, breast, and shoulders, are elevated three feet, while the end of the arms are only a few inches. This structure, differing so widely from all others in this region, is peculiarly strange, unless we can arrive at the conclusion that the animal-shaped mound has been blended in its structure with the one designated as resembling the human figure; and even this one may only represent the same object as is intended by those mounds described as the "citadel," Plate V. Immediately southwest, and within twenty feet of the head of this figure, commences a series



Ancient Remains, Animal Mounds, &c.

of mounds, similar, with one exception, to those which are represented in the plates; for this reason I omit them. The altitude of this one is about eight feet, and its circumference is eighty feet.

Fig. 7 represents an embankment resembling in form the otter or the lizard; its location is upon section nine, near Blue River, and upon the level plain of the English Prairie. The outlines of this figure are very distinct; its elevation along the body is only fifteen inches; bearings, southwest and northwest; head to the southward, and the legs are projecting westward. The length of this figure, from one extremity to the other, is one hundred and thirty six feet and six inches. Figures of this class are frequently found in this region.

Fig. 8, (which may have been intended to represent a bow and arrow, or perhaps the rude sketch of a human figure, or bird with expanded wings,) lies north, and within a few rods of the Wisconsin River, about four miles west of the village of Muscoda. The elevation of this mound is only about one foot, while other figures in its vicinity are very prominent. In the adjacent forest, we find abundant evidence of industry, in the existence of a multiplicity of extensive groups of these monuments of antiquity; and, being in a beautiful and luxuriant district of country, they tend greatly to prove, that a dense population and a powerful people formerly dwelt upon this lovely site.

Plate VII.

Fig. 1 represents an interesting group of earth-works, in the village of Muscoda, (English Prairie,) in the county of Grant, the general direction of which is N. N. E. and S. S. W., beginning upon the bank of a bayou, near the river, and passing through several enclosures. The late cultivation of these grounds has in a measure obliterated and leveled these works, from what may be supposed was their original height; many of them are in the streets and upon the commons; the village in its future increase may cause a complete destruction of these works, so as to obliterate every trace of their shape. In this group are three figures in the form of the cross; in the centre of the largest of these, represented upon the figure by a circle, is quite a depression, occasioned, perhaps, in the same manner and for the same

purpose as described in Plate VI, fig. 1; the outlines of these works are easily traced, although for the reasons above assigned, their elevation at this time does not exceed thirty inches. From the excavations around many of them, it is apparent that they must have been constructed with materials obtained adjacent to them. Some of these mounds, however, seem to have resisted the destructive action of time; those toward the southwest end of the group, are in height six feet; the distance from one extreme of the series to the other, is about four hundred and sixty yards; consequently, in order to include the whole cluster in one plate, it became necessary to reduce the scale, by doing which they appear diminutive, although they are constructed upon a scale equally grand as that of others. The site here is a beautiful level plain of arenaceous loam, being free from trees or shrubbery, (substratum, a fine white saccharoidal sand-rock,) so that a person from the eminence of the most prominent ones, may at a glance view the whole group. Human bones have been found in many of these.

Fig. 2 represents a species of ancient works, which, under various modifications, are very numerous, and comprise about one fifth of the embossed works in this region. This figure agreeing with the location, it being about "one mile from the English Prairie," is probably one of the groups referred to in an article on the subject of antiquities, published in Vol. xxxiv, of this Journal, by R. C. Taylor, Esq., a copy of which, from an esteemed friend, I was fortunate in obtaining, which document, in the absence of other works upon antiquities, has rendered me essential service in my observations upon this intricate subject. In regard to the "group of six," spoken of by Mr. T., I have frequently traversed the forest where they are said to exist, in search of figures having projecting beaks, as represented in Plate II of his article, but I have discovered none approaching that form nearer than that one under consideration.* Those figures near the Blue Mounds and the Four Lakes, which were personally examined by Mr. T., I am happy in saying, are faithfully represented, and many of them I have had the gratification of visiting.

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* It must not from these remarks be inferred, that I have the remotest intention of accusing either Mr. T. or his informant of deceiving the public, by fictitious statements.

The elevation of this figure, as well as those of the group of which it forms a part, about the head, shoulders and breast, is four feet, diminishing toward the extremities, as in other cases, to one foot. Bearings of the trunk, N. 85° E., the head to the eastward. Between the base of the trunk and the southern wing, a mound measuring twenty one feet in diameter and five feet in height is erected. Notwithstanding the grand scale upon which this figure is constructed, there are no indications around or in its vicinity, which would in the least convey an impression that the adjacent earth had been removed, or that the material of which this work was constructed had been taken therefrom. The numerical figures along the dotted lines in the drawing, diverging from a common centre upon the breast, indicate the dimensions of the structure.

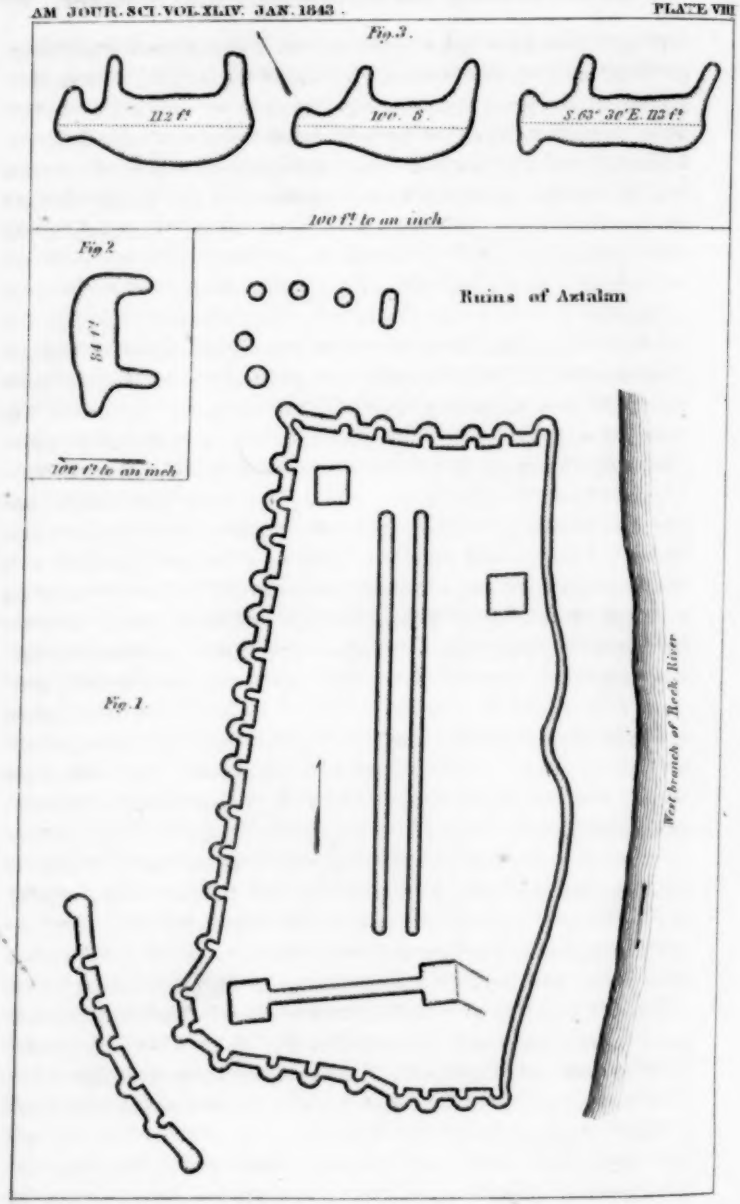
Fig. 3 is the centre one of a series of mounds, fifteen in number, extending the distance of about three hundred yards, and placed at intervals of twenty-five feet apart. It seems apparent that this figure must have been originally constructed as represented in the drawing by the dotted lines, having at these points an elevation of three feet, and that subsequently additional earth was heaped upon the head, breast and end of the trunk, elevating these parts three feet above the other points of the structure, now measuring six feet. Bearings, N. N. W. and S. S. E.; the head to the northward. The site of this figure, upon the northeast part of section thirty-five, north, and within a mile of the Wisconsin River, west of fourth meridian, is a commanding swell in the forest, and were it not for the lofty timber, would be an eligible position for an observatory, having an interesting view of the Wisconsin River for some distance above and below, and of the beautiful English Prairie to the southward, which is baricaded as it were by magnificent bluffs, extending along the river many miles.

Fig. 4 represents the terminating figure of the same series as that of fig. 3, above referred to. This figure, with the exception of the *brush*, resembles the reynard, with drooping tail; that it was really intended to represent that animal, I am not prepared to say. Earth-works of this form are frequently met with in this region, more especially in the forest in Richland County. For its dimensions, the reader is referred to the scale accompanying the

drawing. Bearings, N. 45° W.; the head to the northwest, and the legs projecting southwestwardly; general elevation, eighteen inches.

Fig. 5 is a sketch of the outlines of a curious animal-formed embankment, which lies adjacent to the road, in the vicinity of fig. 8 of Plate VI. The form of this structure is very perfect and well proportioned; its elevation is about three feet, being highest about the middle and toward the back; its length from one extreme to the other is seventy-nine feet, and in width, over the middle, twenty-four feet. Bearings, east and west; the head to the westward, and legs projecting southward. Throughout this region embankments of this form are very numerous, some of which have two parallel projections from the back of the head, while in the present one they seem to be so blended as to represent but one. There is, I believe, in zoological history, no analogy to this figure.

Fig. 6, situated upon and near the east line of section thirty-five, to the eastward and within a mile of fig. 1, Plate VI, represents a human figure having *two heads!* which gracefully recline over the shoulders. This singular figure, so unlike the other works of similar form which have come under my notice, is the most perfect that I have seen; the arms, however, are disproportioned, being much too long; their full length in the drawing, for want of room, I have not delineated; all the whole parts are gracefully rounded; the stomach and breast are corpulent; and the entire structure seems to have retained, as I conceive, its original form through all the dilapidations of time. The perfection of this truly singular and interesting specimen of ancient earthworks, is convincing evidence that the ancient inhabitants of this region were *not* as ignorant of the arts as we have reason to believe the present race of Indians are; their works, however, prove that they possessed industrious habits, even if their labors had been bestowed upon objects of no apparent utility. The dimensions of this figure are as follows: widths, from one arm-pit, over the breast, to the other, twenty-five feet; over the arms, at shoulders, twelve, and tapering to four feet; over the hips, twenty feet; over the thighs, near the trunk, eight, and tapering to five feet; over the figure, above the shoulders, fifteen feet; over each neck eight, and over the heads ten feet: lengths of body, fifty



Ancient Remains, Animal Mounds, &c.

feet; of legs, forty feet; of arms, one hundred and thirty feet; of necks and heads, from termination of dotted line, fifteen feet: elevations at breast, shoulders and abdomen, thirty-six inches; arms, at junction with shoulders, same height, diminishing toward their extremities, where they are but ten inches; the thighs, near the trunk, are twenty, while at the feet or extremities they are but ten inches. Bearings, north and south; the heads to the southward.

Plate VIII.

Fig. 1 represents the most singular group of ancient works yet discovered in western America. This monument of an ancient people is in the form of *mounds*, *embankments*, *buttresses* and *outworks*, which, connectedly, very much resembles a *fortification*, and was in all probability constructed for this purpose, by a warlike people! These antiquities, by common consent, are known as the "ruins" or "city of Aztalan," and they are situated in a flourishing region of country, destined, probably, to have in a few years as dense and powerful a population as we have reason to believe flourished there in former times. This is the same "ancient city," which a few years ago had its day, and in a measure shared the fate of the notorious "moon hoax," in the *press* of our country. The Cincinnati Whig, however, more lenient than many other papers, viewing it as a subject well worthy of scientific investigation, concluded a very interesting article, entitled "the ruins in Wisconsin," with the following paragraph:

"The whole subject is full of interest to all, and eminently recommends itself to the antiquary and the historian, and we hope that it will soon be adequately illustrated; but until we shall have more complete and authentic accounts of these ruins, their position to the points of compass, their extent, plan, material of construction, &c., it will be vain to enter into speculations concerning them. An accurate detail of facts is wanted for the basis of reasonable conjecture. All we can do in the mean while is to correct or expose the unauthorized guesses of ignorance and presumption."

I have never personally examined the "city of Aztalan," although I have been a resident of the mining region since the time it was still under the jurisdiction of Michigan. Having

undertaken an interesting subject, and being desirous of embracing in it, with the antiquities of the western part of the territory, those of the ruins of Aztalan, I could conceive of no better mode of acquiring facts concerning them, than soliciting the aid of a friend, in whose probity and competency every reliance may be placed, to furnish me with a description of those ruins, as well as the character of the adjacent country. He has given me the following interesting account, which, depriving it in a measure of its epistolary form, I will here introduce.

"In answer to your enquiries respecting the ancient remains some years since discovered in the eastern portion of this territory, known as the 'ruins of Aztalan,' I regret that the limited opportunity afforded me of a minute examination of the same, will preclude the possibility of affording you such a description at this time as I could wish; although I have no doubt that a thorough examination would throw much light upon this mysterious subject. One thing I am convinced of, and that is, that I with many others who have looked upon the different accounts which have from time to time appeared, respecting the ancient works in this widely extended valley, as a deception and the ephemeral efforts of a penny-a-liner, have been mistaken, for it is no deception, but is well worthy of attention and investigation.

"Ancient embankments and tumuli of different form and construction are, as you are aware, abundant in many portions of our territory; I have examined many of these remains, but in my different researches have not as yet discovered the objects of the constructions which appeared to me at first view so distinct, or which forced upon my mind so strongly the conviction of the existence hereabouts of a former race of beings more skilled in the arts, than any of the present tribes of savages now known to us, as the appearance of the ancient mounds at and near to the town of Aztalan. The 'ruins' you particularly refer to, are to be found in the midst of a beautiful rolling country, conveniently interspersed with timber, and watered by Rock River and its tributaries; the principal remains, or what has been termed the 'walled city,' is situated in township seven, of range fourteen, east, in the Milwaukie land district. I know not that a better idea of the present appearance of these ruins can be conveyed than by the accompanying sketch, prepared some

since by Judge N. F. Hyer, a gentleman who has taken much interest in reference to this subject, and to whom I am indebted for much interesting matter respecting the same during a hasty ramble over the embankments and mounds, and through the immediately adjacent country. The drawing is faithfully executed; the 'citadel,' as there represented, consists of a brick wall, which at the base is from twenty to twenty five feet wide, at the present time, and, I should judge, about five feet in height; the projections of the wall indicated upon the plat, have certainly the appearance of buttresses, as constructed upon military works at this day; they are constructed also of brick, regularly built at intervals of from two to five rods, and extending beyond the wall about seventeen feet, of the same height as the main wall. The eastern wall, parallel with and immediately upon the bank of the river, is, at this time, but slightly visible, nor are there any appearances of buttresses, as upon the other portions of the wall. In proceeding upon the supposition that these are the ruins of an ancient fortification, we may conclude, that inasmuch as the eastern side was defended from egress by a deep and rapid stream, a wall and buttress similar to the one I have attempted to describe as bounding the western side, would have been unnecessary. The whole area, within the wall, comprises about twenty acres; within the enclosure are a number of square mounds or elevated plains, of the height of fifteen or twenty feet, as I should judge, and perhaps forty or fifty feet square upon the top, while others are of a more conical shape, and from their situation appear as what might now be termed block-houses, or places of *look-out;* that such were the objects of their construction, I am not prepared to say. There is also a distinct ridge running east and west, connecting two of these towers or mounds, as well as two parallel ridges running north and south, and extending nearly the whole length of the enclosure. There is also a cellar and stairway, I am informed, yet visible, descending within the mound of the northwest angle of the ruins; this, in my hurried examination, escaped my notice. I can therefore say nothing respecting it. The same remarks must also apply to the termination of a sewer, which is said yet to be perceived at a bend or angle, about midway in the eastern wall designated on the plat above referred to; this sewer is said to be about three feet below the surface, and arched with stone. Whether through

this sewer water was supplied from the river or not, others can judge. Without the enclosure, and at those points where this work is not protected by the river, are numerous mounds varying from three to twenty-five feet in height, and from twenty to a hundred feet in circumference; and particularly at the southwest angle, there is an embankment forming the arc of a circle, with projections resembling the buttresses represented in the main wall, which requires but little stretch of the imagination to suppose was intended as an outwork for the defence of that particular point.

"In examining one of these mounds, I found the remains of a human skeleton, which had been previously exhumed, although by the action of fire the bones were so completely charred that they readily crumbled to pieces in the hand. I preserved a small piece of the skull, but have somewhere mislaid it or would now send it you.

"One word as to the 'brick wall;' let me not be understood to say that there is in the brick here found any regular appearance of *brick-laying*, as at present practiced. The walls which I examined, and from which at many different points, with a mattock, I broke off specimens, present now the appearance of a mass of *burned clay*.* In what manner at first constructed, there is nothing to indicate, but that the walls and parapets consist of brick rudely burned and prepared with straw, after the ancient mode, the different specimens I gathered bear sufficient witness."

Fig. 2, approaching the form of a bear, is included in that extensive group of antiquities, before referred to, near Blue River, upon the English Prairie. This embankment, being much more elevated than many which I have examined, is about midway six feet in height; and in length, along the dotted line from east to west, eighty four feet; and its greatest width over the body is twenty feet. Bearings, due east and west; the head to the westward, and the legs projecting southward. The whole figure, and the grounds adjacent, are covered with *Corylus Americana* and *Lathyrus albidus*, which shrubs, in this region, are indicative of deep and rich soil.

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* A box, containing specimens of the "burned clay," as well as fragments of rudely platted matting and human remains, in a charred state, dug from these ruins, was some time ago forwarded through the curator, Dr. King, to the National Institution for the Promotion of Science, at Washington, D. C.

Fig. 3 is a group of three animal-formed mounds, which are also upon the English Prairie, about eleven miles to the eastward from fig. 2, and within the limits of Iowa County. The destination of these figures, if I may use the expression, seems to have been toward the northwest, their heads being in that direction; their legs, contrary to the direction of all others which I have examined, are projecting northeastwardly. For a more explicit description of these figures, the reader is referred to the scale upon which they are drawn. In the vicinity of these are many other figures, of various forms and dimensions. To the eastward, at a short distance, commences a series of mammillary mounds, varying from one to two and a half feet in height; these mounds are beautifully and with much regularity ranged at convenient intervals, and extend over a distance of about five hundred yards, terminating abruptly with a huge mound eighteen feet in height, and in circumference at base two hundred and twenty-five feet. To the northward and southward of the *figures*, and parallel with them, are numerous embankments with intervening spaces, representing gateways, a further description of which would only lead to unnecessary repetition.

I am well aware that the actual existence of earth-works as singularly shaped as those represented in the accompanying plates, may be, and even is by many who reside in the midst of them, looked upon with incredulity, which induces me to illustrate one of the many facts which proves that the minds of *all* are not directed alike to the same subject. *Quot homines tot sententiæ*—so, as various as are the opinions, so are the subjects, upon which the tide of their reflections flows. Col. William S. Hamilton, a gentleman who for many years has been a resident of the west, and has for a few years past been engaged in the business of smelting lead in the village of Muscoda, surrounded as it were by these monuments of antiquity, has almost daily passed over many of them without observing their structure, although the outlines of the numerous figures are very distinct; but after having their interesting features pointed out, Mr. H. now acknowledges that what he has heretofore viewed, in the various accounts which have appeared respecting them, as the result of strong imagination or of wild fancy, he had become thoroughly convinced of their singular beauty of construction and of their remote antiquity.

Having now briefly illustrated at least one figure of each species of artificial earth-works, embraced in a scope of country upon both sides of the Wisconsin River five miles in width, and in length fourteen miles, the river passing midway from one extreme to the other; and each figure having been the subject of careful observation, being part of the result of more than twelve months of incessant labor, in rambling in quest and taking admeasurement of about a hundred figures; an anxiety to embrace in this article a variety, has extended the subject and prolonged its completion much beyond what I at first had intended, but should the matter contained in these pages in the least degree aid antiquarians in their researches in Indian archeology, I will be amply rewarded.

In conclusion, fearing that I should have gone too far in hazarding conjectures why or how these antiquities *are as they are*, I ventured to give no conclusive opinion, inasmuch as there have already been too many wild speculations respecting many of them throughout this vast region. I have endeavored only to represent them as they really exist; in doing this it became necessary to take their dimensions in the field with a degree of accuracy, in order that the description may in future serve when all traces of the mounds are obliterated, (which in this fertile region will soon be the case,) as a record of what once existed. The subject is a dark one, and the word *mystery* seems stamped upon every foot-track of those who primitively flourished here. We know not whence they came—we know not where nor how they have departed. A people has passed—a nation has gone away—their history we know not, nor the history of their works.

Forest of Richland, Wisconsin, July 31, 1842.

Art. IV.—*Remarks and Observations on the Formation of Fogs;* by Prof. W. M. Carpenter, M. D.

Meteorologists, in examining into the causes which give rise to the formation of visible vapor, seem generally to have confined themselves almost exclusively to the theory of Hutton, considering it as incident upon the mixture of masses of air of different temperatures, and to have neglected in a very great measure the consideration of other causes. The intention of the follow-

ing remarks will be to render it probable that other influences may, under favorable conditions, give rise to the same phenomenon; and also to show that the prevailing opinions respecting the conditions requisite to its production, are in some measure erroneous.

In the year 1819, a paper was published by Sir H. Davy, in the Philosophical Transactions, giving his views, and the result of his observations on the formation of fogs and mists. The following is a summary of the results at which he arrived. "After sunset, the earth commences to cool in consequence of radiation, but land and water are cooled by this operation at very different rates: the surface of the land cools much more rapidly than that of the water, and the air over the land becomes colder than that over the water, and when they both contain their due proportion of aqueous vapor, and the land is so situated as to permit the cold air from the land to mix with the warmer air over the water, the production of mist or fog will result. The density of such fog will moreover be greater as the land surrounding the water is higher, and the water deeper and warmer."

Here Davy follows Hutton, in attributing the formation of fog to the mixture of masses of air of different temperatures, and all the forms of visible vapor are now supposed to have their origin in some modification of the same influences.

He goes on to say, that in no case that came under his observation, "was fog formed on a river or lake, when the temperature of the water was lower than that of the air over it, even though the air was saturated with vapor." These remarks have been generalized and a law established, which I believe is generally received as true, that "*the formation of fog never takes place over water*, when its temperature is lower than that of the atmosphere, not even though the air should be saturated with vapor." Now we cannot hesitate about considering the observations of Davy as perfectly accurate, but it will be shown that the law is not of universal application, and that we should, consequently, protest against the too hasty generalization.

From a comparison of my own conclusions, drawn from observations on the fogs of our southern rivers, with those of Davy, drawn from observation on many of the great rivers of Europe, I should infer, either that the agencies governing their formation there must be different from those in which they originate here;

or that his observations, particularly on rivers running towards the south, must have been mainly limited to the last of summer, the autumn, or first part of the winter.* For, were the conditions always observed by him to accompany the formation of fogs absolutely requisite for their production, it would preclude the possibility of their occurrence on the Mississippi River during a considerable portion of the year, particularly during the latter part of the winter, the spring, and first part of summer. For, the large body of water which comes down that river at those seasons, from much colder regions, traverses the climates through which it passes so rapidly, that it does not acquire their temperature; and when it arrives here its temperature is always much below the mean of those seasons, and even below the ordinary monthly minima. Convinced that this must be the case, and aware that the water of the Mississippi River is pleasantly cold for drinking during all the spring and first summer months, which required, at those seasons, that it should be considerably colder than the air, I was satisfied that fogs, which are of common occurrence at those seasons, must be formed much more frequently over this body of water, than the temperature of the air was lower than that of the water. In the latter part of the year 1839, I commenced a series of observations with the view of testing the truth of these conclusions. The following table exhibits the results of observations, made during the formation or prevalence of fogs, when the temperature of the water was lower than that of the air.

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* Since the above was written, this inference has been confirmed by reference to Paris's Life of Davy. I give the names of rivers, on the fogs of which he made observations, with the place of observation, the general course of the rivers as to latitude, and the date of observation. 1st. The Rhine, between Cologne and Coblentz; course, north; date, 31st of May. 2d. The Danube, between Ratisbon and Vienna; general course of the waters from the sources, the tributaries being taken into account, northwardly; date, 9th, 10th and 11th of June. 3d. The Raab, near Kermond in Hungary; course, north; date, 11th July. 4th. The Save in Carniola; course, south; date, "end of August." 5th. The Ironzo, in the Friul; course, south; date, "middle of September." 6th. The Po, near Ferrara; course, south; date, "end of September." 7th. The Tiber; course, south; date, "beginning of October." So that only the Ironzo, the Po, and the Tiber, have southern courses to the points of observation, and on all these the observations were made during the autumn.

Date of observations.	Temperature of the water of the Mississippi River.	Temperature of the air at the surface of the water.	Temperature of the air at the height of six feet above the surface of the water.	Temperature of the air on shore.	Time of day at which the fog formed.
1842, Jan'ry 11,	46°	53.			10 A. M.
1841, Feb'y 15,	49.	59.			10 A. M.
" " 25,	50.	57.			11 A. M.
1840, March 27,	53.	61.25	61.75	62.25	3 P. M.
" " 29,	53.	60.	60.50	61.	11 A. M.
1841, April 1,	56.50	61.50			Night.
" " 2,	56.50	64.			3 P. M.
" " 5,	56.50	63.50	63.75	64.25	Night.
" " 19,	57.	71.			10 A. M.
1842, " 13,	54.	66.			11 A. M.
" " 27,	55.	59.50			9 A. M.
" " 28,	55.	63.			Night.
" May 3,	61.50	67.			Night.
" " 5,	61.50	70.			11 A. M.
" " 13,	61.75	67.50			Night.
" " 16,	61.75	74.			2 P. M.
1841, June 19,	69.	76.			10 A. M.

In one of the cases referred to in this table, the opportunities for making the observations were so favorable, the results so satisfactory, and the nature and variety of the phenomena so characteristic of the particular kind of fogs, that I will give a detailed account of the observations.

On the 27th of March, 1840, I was at the house of a friend, who lives on the bank of the Mississippi River. The weather was cloudy and rather cool for the season in this climate, the thermometer ranging from 55° to 65° Fahr., though it had ranged during some time previous at from 65° to 75°. The house was situated on the eastern bank, at the distance of about four hundred yards from the water's edge, the greater part of the distance intervening being a level plain, elevated at the time of observation only a few feet above the level of the water, then at a high stage; but near the house the land rises abruptly, forming a bluff about fifty or sixty feet in height, and on this bluff, near the declivity, the house is situated.

About noon there was a light shower of rain, and it remained cloudy during the remainder of the day. At 3 o'clock, P. M., being on the gallery, which commanded a view of the river, I

perceived that fine wisps of vapor were forming in detached spots over the surface of the water. After examining the thermometer, which hung in a good exposure, I hastened to the water's edge, noted the temperature there, and again over the surface of the river, at the distance of about two hundred yards from the shore, where the temperature of the water was also observed. The results are given in the fourth line of the table, for March 27th, 1840, except that in addition, the temperature at the house on the bluff was 64°, being higher than on the bank.

The fog commenced to form in almost immediate contact with the surface of the water; indeed, when first observed, it had much the appearance of a very fine fleece floating in spots on the water. It thickened very fast, however, and before the observations over the water were concluded, which required perhaps ten minutes, the river was veiled in an impenetrable cloud, which stood over its surface to the depth of about fifteen feet. Although the upper part of the fog was elevated considerably above the level of the banks, it remained stationary over the bed of the river, and did not flow over the adjoining plain at all. The upper surface of the fog was level, and though defined with considerable accuracy, it terminated above in a thin haze; and although we could see nothing of a steamboat which passed near us, when we were on the bank, we had a fine view of all the upper parts of it after we had mounted upon a pile of wood, which raised us above the thickest part of the fog. The fog remained stationary over the surface of the river about an hour, and was then swept off by a brisk breeze which sprang up from the northwest.

The observations given above indicate the absence of those conditions which have generally been considered as requisite to the production of the phenomena under consideration. The temperature of the air was higher than that of the water, and that of the air over land, both on the immediate bank and on the higher lands back, was more elevated than that over the surface of the water. The fog could not have resulted from the mixture of the air from over land with that over the water, as the former was lighter than the latter, and could not flow down to the same level with it; neither could it have been produced by colder air from regions above the surface of the river settling down and mixing with that near the surface; for in this case we

would expect to see the vapor commence to appear at some distance above the surface of the water, instead of in the air in immediate contact with that surface. We can hardly fail to attribute the production of fog in this instance to the refrigeration of the air, in consequence of communicating its heat to the water, and notwithstanding the low conducting power of atmospheric air, we must suppose the extension of refrigeration to depend mainly on the conducting power of the humid air. If the diminution of temperature was confined to the stratum of air in immediate contact with the aqueous surface, the deposition of water on that surface would be the only result; but as the chill becomes general through the air above the river, fog will be formed, and will increase in depth and density as the impression is augmented.

The general conclusions at which I have arrived in reference to the occurrence of fogs over the Mississippi River, are as follows:—

1. That fogs form over this river at all seasons.

2. That they occur frequently during the day-time as well as at night.

3. That those occurring during the day-time, though often very dense, are not so extensive as those occurring at night, and rarely extend over land at all.

4. Fogs originating in day-time are of much more frequent occurrence from February to May than at other seasons, though the nocturnal fogs of winter frequently do not disappear before 10 or 11 o'clock in the morning.

5. In a large majority of the observations made by me, during the prevalence of fogs on the Mississippi River, in autumn and winter, the water had a higher temperature than the air. These have been exclusively nocturnal fogs, which frequently extend over large regions of country.

6. In almost every instance in which I have observed the formation of nocturnal fogs over the Mississippi River, during the spring and earlier part of the summer, the river water has had a lower temperature than the air. These vary in extent, without any apparent connection with the relative temperature of the water and air; and the air over the higher lands is by no means invariably colder than that over and near the surface of the water.

7. In every instance in which I have observed fogs to form over the Mississippi River, in day-time, during the spring and

earlier summer months, the temperature of the water was lower than that of the air; and the air over the land warmer than that over the water. These rarely extend over land much, and are most frequently confined to the air over the bed of the river.

8. In all the diurnal and in most of the nocturnal fogs of the spring and first part of summer, observed by me, on the Mississippi River, the production of visible vapor commenced near the surface of the water, and increased in depth by additions above.

It appears therefore that the fogs of the Mississippi River may be considered under two heads:—1st. Those which originate when the water is warmer than the air. 2d. Those which originate when the water is colder than the air. The first kind prevails mainly during the fall and earlier part of the winter; the second kind are most common during the spring and early part of the summer. The first unquestionably results from the condensation of aqueous vapor, which rises constantly from the warmer water into the air, in quantities more than sufficient to saturate it at its temperature. The second, it appears to me, must result from the condensation of aqueous vapor already in the air, in consequence of the extension of refrigeration, from the cold surface of the water, through the warm and humid air above.

Among fogs which form over land, likewise, a large number have their origin independent of the causes assigned by the theory of Hutton. During almost every portion of the year, the high temperature of the air and earth during the day-time, increases the quantity of watery vapor in the air, and the dew-point temperature rises towards the hottest part of the day, so that the complement of the dew-point, except in winter, is rarely more than 10° or 15° Fahr., and most frequently not more than 5° or 10°. The temperature of the air gradually declines until about sunset, while the temperature of the dew-point remains almost fixed; thus diminishing the complement. After sunset, however, the earth cools rapidly by radiation, and the air participates, though not to the full extent, in the diminution of temperature.* It is not unusual to see the temperature of the air diminished by 15° or 20° between 3'clock P. M. and midnight, and

* "In every calm, still night, the air nearest the earth is colder than that which is more distant from it, to the height of at least 220 feet, this being the greatest height to which M. Six's experiments relate."—*Wells, on Dew, p.* 95.

a thermometer, with the bulb blackened, exposed, would indicate a still greater reduction for that of the earth; while the complement of the dew-point of the preceding day was not more than 5° or 10°. If the temperature of the earth alone suffered diminution, the effect would be limited to the deposition of dew, but at the same time that dew is being deposited upon the cooled surface, thus reducing the temperature of the dew-point, the temperature of the air is also diminishing, and it depends altogether upon the relative amounts of the two concurrent decrements, whether any of the atmospheric water will take the form of visible vapor in the air or not. My observations, though not sufficiently numerous to enable me to make a satisfactory application of mathematical formulæ to the results, will still enable us to come to some safe general conclusions on these points. When the diminution of atmospheric temperature and of the temperature of the dew-point depends on the influence of a cold surface, the former seems always to decline most rapidly. This may, I think, be accounted for if we observe upon what the rapidity of diminution in each case depends. The decrement of atmospheric temperature depends for its value—first, upon a certain amount or radiation to the cold surface; second, upon the conducting power of the air, that is, on its degree of humidity, or in other words, upon the temperature of the dew-point; third, upon the difference between the temperature of the air, and that of the refrigerating surface. The decrement of the dew-point temperature will depend—first, upon the deposition of dew, that is, it will be nearly as the amount of humidity; and second, upon the difference between the temperature of the dew-point and that of the cold surface.* If therefore we take c to represent the temperature of the surface, and φ and φ' to represent respectively the difference between the temperature of the surface and that of the air and dew-point, we shall have the temperature of the air $=c+\varphi$, and that of the dew-point $=c+\varphi'$; and if β be taken to represent the effect of radiation, we shall have the decrement of atmospheric temperature proportionate to $\varphi(c+\varphi')+\beta$; while the decrement of the dew-point temperature will be as $\varphi'(c+\varphi')$.

* These suppositions, though not strictly accurate, are sufficiently so for the present purpose, as the variations from the truth affect somewhat equally the two decrements.

Therefore the decrement of atmospheric temperature will be greater than that of the dew-point temperature, by $(c+\varphi')(\varphi-\varphi')+\beta$. If it be denied that radiation has any influence in this case, we still have the decrement of atmospheric temperature greater than that of the dew-point temperature, by $(c+\varphi')(\varphi-\varphi')$. Now this concurrent diminution, by unequal decrements, that of the atmospheric temperature being greatest, tends to reduce this temperature to equality with that of the dew-point; and after the two temperatures have in this way become equal, any farther diminution in the same ratio must result in the production of visible vapor in the form of fog. This explanation, if correct, will apply equally to fogs which form over cold waters, and many which form over land. Over land they occur most frequently towards morning, and are preceded, and perhaps at first accompanied, by the deposition of dew. They are most common in autumn and spring. When they form over large fields, or over our extensive prairies, they first begin to appear as a thin haze near the ground, and become deeper by additions above; but in dense forest regions, the condensation first commences about the tops of the trees, which taken together constitute in fact, in the present instance, the radiating and refrigerating surface.

When heavy and extensive fogs are dissipated in the morning by the heat of the sun, the visible is converted into invisible vapor, and becomes a part of the atmospheric air; but this change is not usually permanent, for the mixed atmosphere is heated, and rising in consequence of diminished specific gravity, it soon arrives at a region where the temperature is as low as its dew-point, when it again takes the form of visible vapor. Clouds thus produced, very often succeed to heavy fogs in this country during spring and autumn; they are generally formed at very short distances above the earth. Often, indeed, the vapor does not lose the visible form at all, but rises slowly until it arrives at a small elevation, when it commences to move as an ordinary cloud. This horizontal motion commences, generally, when the vapor has risen to the height of from one hundred to three hundred feet, but this region of clouds attains a greater elevation as the day advances, until it reaches the ordinary height. This kind of cloud is generally carried over this part of the country, towards the north or northwest, and sometimes continues to pass over during the earlier part of the day, until 10 or 11 o'clock, A. M.

They are not very dense, having rather a hazy appearance, but are very continuous; and it rarely rains from them while they retain this form.

These clouds, as well as the fogs from which they originate, exercise a highly beneficial influence on vegetation; the former protecting it from the direct heat of the sun, while yet moist from the dew and fog of the preceding night; and the latter, by putting a check upon nocturnal radiation, thus lessening the fluctuations of temperature.

Jackson, La., May 9, 1842.

ART. V.—*Abstract of a Meteorological Register, kept at Jackson, (Louisiana,) Lat.* 30° 51′ 25″ *N., Long.* 91° 9′ *W. of Greenwich, during three years ending Jan.* 1, 1842; by W. M. CARPENTER, M. D.

YEARS.	THERMOMETER.										BAROMETER.					
1839. 1840. 1841.	*Mean Temperatures.*															
Months.	Sunrise.	2 P. M.	Sunset.	Monthly mean of diurnal temperature.	Monthly mean of nocturnal temp.	Average mean per month.	Maximum.	Minimum.	Range.	Max. temp. sun's light. Bulb therm. blackened.	Monthly mean.	Monthly average of diurnal maximum.	Monthly average of diurnal minimum.	Monthly maximum.	Monthly minimum.	Range.
January,	45.0	51.5	49.0	48.2	47.0	47.6	74	18	56		29.89	30.00	29.78	30.20	29.50	.70
February,	47.2	53.4	50.0	50.3	48.6	49.4	74	20	54		29.90	30.03	29.78	30.15	29.30	.85
March,	52.2	64.-	58.0	58.2	55.1	56.6	75	33	42		29.83	29.94	29.73	30.50	29.40	1.10
April,	62.3	71.5	65.5	66.9	63.9	65.4	82	54	28		29.92	29.95	29.90	30.12	29.70	.42
May,	67.6	75.7	72.4	71.6	70.0	70.8	85	54	31		29.92	29.94	29.89	30.10	29.70	.40
June,	74.5	84.5	81.3	79.5	77.9	78.7	90	64	26	157.5	29.90	30.00	29.80	30.00	29.80	.20
July,	77.5	88.0	84.0	82.7	80.7	81.7	94	73	21	164.3	30.00	30.02	29.98	30.10	29.78	.32
August,	75.0	86.5	83.3	80.7	79.1	79.9	91	66	25	156.	29.92	29.94	29.90	30.06	29.82	.24
September,	70.0	82.2	78.0	76.1	74.0	75.1	86	59	27	137.	29.91	29.93	29.89	30.05	29 76	.29
October,	63.4	73.0	70.0	68.2	66 7	67.4	85	29	56		29.91	29.95	29.87	30.10	29.69	.41
November,	48.0	54.0	50.0	51.0	49.0	50.0	77	25	42		29.87	29.89	29.85	30 03	29 70	.33
December,	46.0	52.3	49.5	49.1	47.7	48.4	67	18	49		30.00	30.05	29.96	30.20	29.90	.30
Annual mean,	60.7	69.7	65.9	65.2	63 3	64.2					29.91					

Mean temperature of five years, compared with the temperature of well and spring water of the same neighborhood.

Years.	Annual temperature.	Mean temperature of water of wells eighty feet deep.	Mean temperature of the water of the coldest springs.
1837,	64.18		
1838,	64.27		
1839,	64.14		
1840,	64.23		
1841,	64.28		
Mean,	64.24	64.25	64.25

The water of our wells and springs is derived from extensive strata of sand, lying above, and sometimes between, beds of clay. These strata deviate but slightly from a horizontal position, and as the surface of the country is undulating, the same bed will be reached at different depths at places near together, so that wells are often seen twenty or thirty feet deep, or springs rising from hillsides, having water of the same temperature as wells in the same neighborhood having a depth of eighty feet. When the average depth of a stratum below the surface is seventy or eighty feet, the variation of temperature of the water of wells through the year is scarcely perceptible, and the annual mean is about 64.25°. But in wells bored to strata whose mean depth below the surface is not more than twenty five or thirty feet, the variation is often several degrees, and the mean afforded not lower than 67° or 68°.

ART. VI.—*Some Remarks on the methods in common use of obtaining the Mean Temperature of Places, and on the supposed difference between the Temperature of the Air and that of the Earth;* by Prof. W. M. CARPENTER.

IT is stated by Humboldt and others, that the mean temperature of the coldest springs in warm climates is often lower than that of the air of the same places. If we examine those agencies in which atmospheric temperature originates, and by which terrestrial temperature is modified, we shall perceive that such a condition could not exist, and consequently, that the observations on which such conclusions were based, were not accurate, or that some unsuspected agency must modify the relations which should otherwise be constant. In examining the meteorological records of our own country and of other parts, and comparing the observations made by different persons resident at the same places, we shall perceive that the results differ not more from the mean temperature of the place, than from each other. These discrepancies, or rather these departures from accurate results, are dependent on many circumstances. In the first place, thermometers of very inferior quality are in very general use, and they will often differ in the results afforded by four or five degrees. In the next place, sufficient importance is not attached to the position of the instru-

ment; and instead of having it completely protected from reflection, and in almost complete obscurity, taking care at the same time to secure a free circulation of air about it, we find them often in such situation as to receive from walls or the earth a considerable portion of reflected heat; often in closed apartments or against walls, the temperature of which is influenced throughout the day by the full force of the sun. Galleries fronting the north are favorite places for suspending thermometers, under the impression that no heat is reflected from that side. In this way we often find, in this climate, that when a thermometer properly placed gives a temperature of 90° or 91°, all the others in the vicinity will stand at 95°, 100°, or even 110°; so that the annual mean derived would be very greatly above the true one. There is, however, it appears to me, a source of error much more general: I speak of the methods of calculating the mean after the observations have been made, and of the adoption into general use of methods which have been found to give correct results at particular places. The methods most in use are the following, viz. to take three observations, and from these calculate the mean directly; some fix upon sunrise, 2 P. M. and sunset; others, as in the Army Meteorological Register, and in those of most of the meteorological societies, upon 7 A. M., 2 P. M. and 9 P. M. I have ascertained, by taking the mean of hourly observations made here by myself, that any of these methods give means which, during every season of the year, are too high, and that the error in excess is greater, in proportion as the diurnal exceeds the nocturnal temperature. The last method, which is now in most general use, has probably been adopted in colder climates, as agreeing experimentally in its results with those derived from more frequently repeated observations during the same period; or possibly under the impression, that the 9 P. M. observation would give an approximation to the nocturnal mean, while the diurnal temperature would result as a mean from those at 7 A. M. and 2 P. M. In either case this method might be the best adapted for many climates, but as the relative value of the temperatures at the times fixed for observation are not constant, but will vary in passing from one climate to another, there is nothing in the method to suit it for universal use. The fact is, it appears to me almost impossible to fix upon any method, except that of hourly observations, which will in every place afford even an approxi-

mation to the truth, or obtain harmonious results from elements the relations of which are so variable as those of the temperatures of particular hours.

The method which I have adopted, and had in use about three years, I have found experimentally to give results, for this climate, almost in precise accordance with those derived from calculations based upon hourly observations. I have constantly kept daily observations at 6 and 7 A. M., 12 M., 2 P. M., sunset, and 9 P. M., so that I have been able to give my observation any of the common tabular forms, and have always obtained the general results according to each of the methods, and the results have verified my previous conclusions by varying from each other as follows: the mean annual temperature of this place, by my method, is 64.24° Fahr.; as derived from the sunrise, 2 P. M. and sunset observations, 66.30°; and during the same period, the mean obtained from the 7 A. M., 2 P. M. and 9 P. M. observations is 65.62°. In this way I have compared results by each method with each other, and with those obtained from hourly observations. I have found that the following method gives for this place results more accurate than any other I have been able to devise. The mean temperature of the diurnal portion of the twenty four hours is derived as the mean of the highest and lowest temperatures of that portion, that is, of the sunrise observation and that of 2 P. M. The nocturnal mean will in like manner result as the mean of the highest and lowest temperatures of that portion, that is, of the sunset and sunrise observations. The average of these two means will give the mean for the twenty four hours. By this method, the sunrise temperature being the lowest for the twenty four hours, will belong to both the diurnal and nocturnal portions, and will enter twice into the calculations, while the other two observations will represent the maxima of the portions of the twenty four hours to which they respectively belong.

If it is as I suspect then, that the results of observations and calculations are, particularly in hot countries, frequently too high, a little attention to the subjects above hinted at may show that there is really no difference between the mean temperature of climates and of water derived from such depths as not to be affected by the change of seasons. In my tables the most exact agreement is shown between these temperatures at this place. The same appears to be the case in the island of Cuba, notwithstand-

ing that we find the following statement in the table in which Prof. Kupffer compares the annual temperatures of places with that of the earth: "Havana, temperature of the earth, 74.30°; of the air, 78.12°." Now Havana and Matanzas are in the same latitude, or nearly so, and there can be but little difference between the temperatures at the two places. Mr. A. Mallory, in this Journal, Vol. xxxi, p. 289, gives the annual temperature at Matanzas as 77.06° Fahr., which he derives as a mean of the following averages of observations: sunrise 72.17°, 2 P. M. 81.41°, and sunset 77.61° Fahr. Now a mean of these observations would unquestionably give a mean too high by more than a degree, which would reduce the mean down to about 76°. My friend Mr. W. H. Potter has been kind enough to examine the temperature of wells, &c. for me, during a residence of a year in that island, particularly near Cardanus, in the same latitude and near Matanzas, and the observations frequently repeated through the year; and the temperature, apparently invariable, was 76° Fahr., thus affording a probability at least, that there also the mean temperature of the air, and of the earth at a certain depth, is the same.

What inferences might not be drawn by travellers in this country, particularly if they belonged to the anti-Huttonian school of geologists? I will not go out of my own neighborhood. The temperature of water taken from strata whose average depth below the surface should be seventy or eighty feet, would be found to have at Baton Rouge a temperature of about 64.50° Fahr. But we are informed by the tables of the Army Meteorological Register, that the annual mean for that place is 68.07°, making a difference of 3.57°. Now we cannot mistrust the accuracy with which the observations were noted in this case, nor the correctness with which the calculations were made, but there can be no doubt that too high a temperature has been obtained for want of attention to things generally considered as of minor importance, and in consequence of the too general adoption of methods which have been found to give correct results in other climates.

Jackson, La., May 9, 1842.

ART. VII.—*Notices of some Trappean Minerals found in New Jersey and New York;* by Prof. LEWIS C. BECK.

ABOUT three years since, I published in this Journal* a notice of some of the minerals, principally copper ores, found in New Jersey. I have continued these investigations during my leisure hours, and have been particularly interested in this work in consequence of the identity of some of these minerals with those of New York, in the study of which I have been engaged for six years past. The present communication will be confined chiefly to such as belong to the zeolite family, to which, however, the more general term *trappean* will better apply.

Short notices of some of these minerals, with reference to the New Jersey localities, will be found in Cleaveland and other mineralogical works; and some details are given concerning them in Prof. H. D. Rogers's Report on the Geology of New Jersey. They occur in greater or less number in many of the trap and greenstone ranges which traverse that state. Of these, Bergen Hill, near Jersey City, has hitherto afforded by far the greatest number of species, as well as the most characteristic and beautiful specimens. A particular account of this locality has been given by Mr. William O. Bourne in Vol. XL, p. 69, of this Journal, and he must have been very successful in his explorations, as his list of minerals sufficiently proves.

My attention was called to the Bergen Hill minerals in 1838, by receiving a collection of them from my friend, the Rev. John L. Janeway, who informed me that he had frequently obtained specimens previously to that time. I have since then made frequent visits to this locality, but I cannot boast of any thing like the collection described by Mr. Bourne. Some of the minerals which I have obtained, however, seem to be of sufficient interest to warrant more than a mere passing notice, and I therefore proceed to the immediate subject of this paper.

STELLITE.—This is the name given by Dr. Thomson† to a mineral found in the rifts of a greenstone rock situated on the banks of the Forth and Clyde canal in Scotland. A mineral, which, for reasons to be shortly presented, I suppose to be identical with the stellite, has been found in considerable abundance

* Vol. XXXVI, p. 107. † Outlines of Mineralogy, Geology, &c., I, 313.

in similar rifts in the greenstone at Bergen Hill. I have also found the same mineral, although less distinctly characterized, in the greenstone at Piermont, the *terminus* of the New York and Erie railroad. This mineral occurs in veins of various degrees of thickness, from an inch to that of a mere coating on the greenstone to which it is attached. In every case, however, the mass is made of fibres or crystals the form of which cannot be exactly determined, usually radiating from various centres, and interlacing each other in an irregular manner. Sometimes these fibres are parallel, when the mineral bears a resemblance to the nemalite of Nuttall. When the bundles of fibres are broken up, they separate into needle-form crystals, which are exceedingly sharp. These needles are tough, and when subjected to the pestle appear like amianthus. The same amianthoid appearance is exhibited by many of the specimens found at the locality, a change which may perhaps be referred to atmospheric agencies.

Color snow-white. Lustre silky, shining. Translucent. Hardness about 3. Specific gravity of a closely aggregated specimen, 2.836.

Alone before the blowpipe, our mineral fuses readily, even when in quite large fragments, and with slight bubbling, into a beautiful white enamel. With borax it melts into a transparent glass. It gelatinizes with muriatic acid. When heated to redness in a crucible, it forms a porcelainous mass from partial fusion. According to my analysis, the following is its composition, in one hundred grains, viz.

Silica,	54.60
Lime,	33.65
Magnesia,	6.80
Oxide of iron, with a little alumina,	0.50
Water and carbonic acid,	3.20*

Our mineral differs from the stellite of Dr. Thomson in its containing less alumina, oxide of iron, and water; but these may,

* The composition of the Scotch mineral, according to Dr. Thomson, is as follows, viz.

Silica,	48.465
Lime,	30.960
Magnesia,	5.580
Alumina,	5.301
Protoxide of iron,	3.534
Water,	6.108

after all, be accidental ingredients. The ratio of silica and lime, which are the essential constituents, is nearly the same. This mineral bears a close resemblance to Wollastonite, as described by Dr. Thomson; and the proportions of silica and lime are also nearly the same. But Wollastonite contains from nine to ten per cent. of soda. Moreover, in most of its properties, our mineral bears a greater resemblance to the zeolite family than any other; and if so, the only species with which it at all agrees in its composition is that above mentioned.

Mr. James D. Dana in his work on mineralogy remarks, that the stellite of Dr. Thomson is closely allied to natrolite. In answer to this, it is enough to say, that natrolite contains twenty four per cent. of alumina and sixteen of soda. Indeed, nearly all the minerals of the zeolite family contain from twenty to thirty per cent. of alumina; and none of them, I believe, except stellite and apophyllite, contain less than eight or ten per cent. of that earth.

Thomsonite, for which the Bergen Hill mineral has been quite generally mistaken, differs from it in its composition and in its behavior before the blowpipe. Thomsonite swells up like borax, becomes opaque and snow-white, but does not melt; on the other hand, all the specimens of supposed stellite from Bergen Hill melt quite easily into a white enamel. I will not say that Thomsonite has not been found, or does not occur, at Bergen Hill; but I have not met with it in any of the specimens which I have obtained from that locality.

Apophyllite.—This mineral, which is very well described in standard works on mineralogy, occurs at Bergen Hill and Paterson, in greenstone and trap, and at Harlem, New York, in gneiss. The primary form, which is often found at the above localities, is a right square prism. It differs from the other minerals which it resembles, in cleaving easily in the direction of P, the cleavage presenting a high pearly lustre. The faces of the prism have a vitreous lustre, and are often striated longitudinally. The crystals are usually transparent, but occasionally they are entirely opaque and white; and in one of my specimens the change from transparency to opacity seems to be going on in the cabinet.

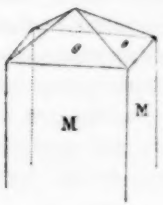
Before the blowpipe this mineral exfoliates and finally melts into a porous glass. The crystalline forms observed at the above localities are in all respects similar to those figured in elementary

works. (Vide Shepard, Vol. II, p. 37.) Most of them in my specimens are quite small, although very distinct. My largest crystal is a little more than a quarter of an inch in diameter.

Mesotype.—I introduce this mineral here because it is apt to be mistaken for the preceding, with which it is often associated. As there is some confusion in regard to this species, it may be proper for me to state that this notice applies to the mineral called mesolite by Dr. Thomson and others, which I have observed only in the gneiss of Harlem, although according to Mr. Bourne, it has been found quite abundantly at Bergen.

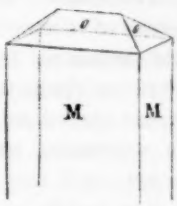
The mineral to which I refer, occurs in very minute but highly finished transparent crystals. The prisms appear to be rectangular, but on inspection they will be found to be slightly rhombic. The terminations, which are usually four-sided, are variously modified, as in the annexed figures. All these forms result from the different degrees of extension of the planes *o*.

Fig. 1.



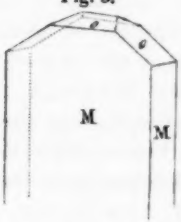
Harlem, N. Y.

Fig. 2.



Harlem, N. Y.

Fig. 3.



Harlem, N. Y.

M on M, 91° 20′
M on *o*, 116° 32′, *Haüy.*
116° 58′, *Phillips.*

Analcime.—This mineral occurs in small crystals of the leucite form at Bergen Hill, where it is associated with stellite, datholite, &c. It has long been credited to Paterson, N. J. I have also found crystals of the same form in the gneiss near Yonkers, in Westchester county, N. Y. The crystals are often

quite opaque. I have not observed any other form than that usually figured.

Datholite.—This mineral has been found in abundance at Bergen Hill. It also occurs at Paterson, N. J., and at Piermont, Rockland county, N. Y. It may generally be distinguished by its high vitreous lustre. It is often associated with calcareous spar, having a highly modified form. The action of an acid and the blowpipe will of course readily decide the question.

In the flame of a lamp datholite swells up like borax and becomes opaque, in which state it may be crushed by the fingers. In hot nitric acid it is dissolved, leaving a siliceous jelly, which adheres to the flask. The crystals are usually transparent or translucent, but the forms are so highly modified as to baffle all my attempts to figure them.

Chabazite.—This is of rare occurrence at Bergen Hill, and the crystals, which are in all cases of the primary form, are very minute. In one of my specimens, the rhombohedrons are an eighth of an inch in diameter. There occur at this locality small obtuse rhombohedrons of calcareous spar, which might at first sight be mistaken for chabazite. But the lustre and cleavage, independently of the chemical characters, will sufficiently distinguish them.

The crystals are translucent or opaque, sometimes white, and at others of a brownish tint. The edges and angles are sometimes rounded, as if they had undergone partial fusion subsequently to their formation.

Prehnite.—It is abundant in various parts of New Jersey, especially at Paterson, Scotch Plains, and Bound Brook. About a year since, Dr. Houghton, the geologist of Michigan, and myself, discovered a vein of prehnite in the trap on Bound Brook, about eight miles northwest of New Brunswick, from which we obtained an abundance of the mineral; but the specimens do not possess much beauty. It has been found at Bergen Hill, but not abundantly. I have also identified it in the greenstone at Piermont, Rockland county, N. Y.

Most of the specimens from New Jersey which I have examined, are of a light green color, and are made up of globular or reniform masses, on the outer surface of which sections of crystals both of the primary and secondary forms may be observed. These crystals, however, are soon lost in radii which pass to the centre. This radiated arrangement may be observed in almost all the specimens.

Occasionally the masses of trap at Bound Brook have a thin coating of white or bluish white prehnite, in which the radiated arrangement has been exchanged for one that is indistinctly plumose. Sometimes, however, it is nothing more than a thin scale.

Prehnite froths before the blowpipe, and when the heat is continued, it melts into a compact colored globule. This globule is usually light green, but in a specimen from Piermont, it is nearly black, a fact which at first led me to doubt whether it was really prehnite. But I have no knowledge of any mineral with which it agrees so well as with this.

Stilbite.—This mineral occurs at Bergen Hill, Paterson, Bound Brook, and elsewhere in New Jersey. It is also found at Harlem, and in the counties of Westchester, Putnam, and Rockland, N. Y. The primary form has been met with in Putnam county, but the most common form in New Jersey, so far as my observation extends, is that represented by the accompanying figure.

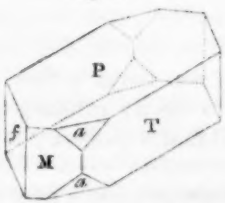
Fig. 4.

Bergen Hill, N. J.

The crystals are often aggregated in various ways, sometimes in the form of a sheaf. This is more common in those cases where they occur in cavities in the trap or greenstone, and it is then often difficult to determine the exact crystalline form. The mineral varies from transparent to opaque, and the color is white, yellowish, or red. The face M has a pearly lustre, but it is not so perfect as the face P in the next species.

Heulandite.—This often accompanies the preceding species, which it closely resembles in many cases. The splendent pearly lustre of the base of the prism will in general serve to distinguish it. This character is very marked in the small but quite perfect crystals found at Paterson and Bound Brook. At the latter locality I have noticed the primary form and the secondary, herewith figured.

Fig. 5.



Bound Brook, N. J.

M on T, 130° 00′
M on *a*, 146° 30′
T on *a*, 148° 00′
P on *a*, 111° 56′
M on *f*, 114° 20′

The crystals are often so much compressed as to resemble four or six-sided tables. Their color is usually white, and they are either translucent or opaque. The opaque crystals exhibit the pearly lustre in the highest perfection.

Besides the minerals already noticed, there have been found at Bergen Hill specimens of crystallized iron pyrites and galena, besides calcareous spar exhibiting various crystalline forms. Among these may be mentioned the *métastatique* and the *cuboide* of Haüy. The latter, as I have already observed, may easily be mistaken for the primary of chabazite. In other cases these crystals are modified so as to resemble some of the forms of datholite; and in a few instances they even have the high lustre of this mineral. I have indeed thought that they might be pseudomorphs. Several specimens of this kind, which I took for chabazite and datholite, were dissolved with effervescence in dilute muriatic acid. I will only add, that at Bound Brook and Paterson, agate balls occur in the trap. These are usually quite small and unimportant, but beautiful specimens, two or three inches in diameter, were several years since obtained from the latter locality.

ART. VIII.—*A short Notice of the Action of Potash upon Cholesterine;* by J. LAWRENCE SMITH, M. D., of Charleston, S. C.

FOR some reasons we would be induced to place cholesterine among the fatty bodies, but from many of its characters, it would appear certainly not to belong to this class of bodies. The most important distinctions between these two bodies are—first, the want of action of a solution of potash upon cholesterine; and secondly, its high point of fusion, which is 298° Fah.*

Another difference which I am able to point out is, that cholesterine is heavier than water, whereas the fats are lighter. It will be found in works on chemistry, that cholesterine is lighter than water, and I attribute this to the fact, that the substance, as it crystallizes out of alcohol, was found to float on the surface of water; but this is owing to the air adhering to the crystals. To show that it is heavier, all that is necessary to be done, is to

* The melting point of most of the fats is below 140°.

throw a small piece of fused cholesterine into a vessel containing water, that must afterwards be made to boil, (this is done to drive away the air adhering to the surface of the body,) after which it will be found to sink, and remain at the bottom of the vessel even when the water is cold. I dwell thus much upon this, because I feel confident that there are other organic bodies that are said to be lighter than water, but which are actually heavier; for owing to the looseness of their structure, air insinuates itself between the molecules, and is afterwards held so firmly, that it is impossible to drive it away by the ordinary means. I now return to the first distinguishing character between cholesterine and the fats—the difference of the action of potash upon the two bodies.

Chevreul and others have shown, that if cholesterine be digested a great length of time in a boiling solution of potash, no change takes place; but here the cholesterine is not subjected to the action of the potash under the same circumstances as the fats; for in the case of the latter, the point of fusion being considerably below that of boiling water, the force of aggregation is in a great degree destroyed, and consequently does not oppose itself to the chemical action; whereas in the case of cholesterine, its point of fusion being much higher than that of boiling water, it remains solid, and therefore its force of aggregation opposes itself strongly to the action of potash, (supposing one to exist.) So then, the difference of the action of a solution of potash upon these substances, is not such a strong mark of distinction as it would at first sight appear to be, as it is impossible to subject them to this action under similar circumstances.

This fact is mentioned, not to show that cholesterine may be a species of fat—far from it; it is simply to attempt to exhibit, that there is no stronger reason for supposing that cholesterine is not a fat, because a boiling solution of an alkali does not act upon it, than there is for considering spermaceti a fat, because it is acted upon; as here the spermaceti is in a state of fusion, one that is favorable to this action, and the cholesterine solid, a state opposing this action.

In an article on spermaceti published in this Journal, Vol. XLIII, p. 301, I stated my reasons at large for not believing this body to be a fat, properly speaking, and at the same time explained how I supposed an alkali to react upon it; it was there ranked

with athal and cholesterine. I then also stated, that although a boiling solution of an alkali might not react upon cholesterine, still I had no doubt that the alkali by itself, aided with a high temperature, would react upon it in a manner similar to that which it did upon spermaceti. From the kindness of M. Pelouze, who furnished me with a small quantity of cholesterine, I have been able to examine into the truth of this supposition.

The first circumstance necessary to be observed in the examination of this reaction, is to have the cholesterine intimately in contact with the potash, and this is done by rubbing together equal parts of the two substances in a mortar. The mixture was placed in a watch-glass, and spread out so as to expose a large surface to the air, the watch-glass was placed on a support in a copper vessel, (the air contained in this vessel could be brought to any required temperature;) the experiment being thus disposed, the vessel was heated, and by the time that the air in the interior arrived at 248° Fah. a change began to take place in the mixture, and at 266° Fah. it was of a dark brown color.

This was now treated with cold ether, which dissolved the unaltered cholesterine, and also a matter of a resinous character, which when dissolved in alcohol, and the alcohol allowed to evaporate spontaneously, is deposited in the form of little round concretions entirely devoid of crystalline structure; it is not soluble in any of the alkalies. What remains after the treatment by ether, is of a brown color, and completely soluble in water. If hydrochloric acid be added to this solution, it is decomposed, and a yellowish substance arises to the surface. This substance is soluble in ether, alcohol, potash, soda, and ammonia, as well as their carbonates; it does not crystallize, its alcoholic solution reacts slightly acid upon litmus paper. In fact it is an acid of a resinous character; its combinations with alkalies have the character of soaps. Its silver salt is of a yellow color, but soon becomes black by exposure to the light.

From the small quantity of cholesterine that was at my disposition, I have not been able to obtain sufficient of the acid to examine its composition, but I have no doubt that it is a new one.

If the mixture when heated be not well exposed to the air, very little of this acid is formed, even if we elevate the temperature as high as 300° Fah.; but on the contrary, a considerable

quantity of the resin before mentioned (soluble in ether) is formed; this though is capable of being converted into the acid, by the action of potash, a high temperature, and free access of air. Thus then it will be seen, that the action of potash, instead of being a means of showing that spermaceti and cholesterine are two substances of entirely different natures, affords strong evidence of their being similar bodies. Further, the action of potash upon spermaceti, is to produce athalic acid and athal, the former capable of forming soaps with the alkalies, and the latter of being converted into the former by an alkali and a high temperature.

The action upon cholesterine is to form an acid (which it is impossible for me as yet to name) and a basic resin; the former forms soaps with alkalies, and the latter by the action of potash at a high temperature is converted into the former.

This article is meant as an appendix to the one on spermaceti, and as an additional proof of the analogy that exists between that body and cholesterine, they being two of a class of bodies which will no doubt be found to be tolerably numerous, and which class I propose to call *pseudo gras*. Among them may be mentioned spermaceti, cholesterine, athal, ambreine, and probably stearérine and elaïérine, two fatty substances found in linseed oil, and which M. Chevreul brought to the notice of the Academy of Sciences not long since. This class of bodies would appear to be a link between the fats and resins.

Paris, July 8, 1842.

ART. IX.—*A Review of the Researches upon the Dilatation of Gases, by M. V. Regnault;* by J. LAWRENCE SMITH, M. D.

On the dilatation of atmospheric air.—"There does not exist in physics a numerical element that has been submitted to so many experimental determinations, as the coefficient of the dilatation of air; and nevertheless we cannot say that this coefficient has been determined with sufficient precision.

"The beautiful experiments of Gay Lussac upon the dilatation of gases, seemed to have completely decided this question. He showed by a great number of experiments that the coefficient of dilatation between 0° and 100° Cent. was the same for all gases and vapors, when they are not extremely near their point of con-

densation, and that its value was 0.375. This coefficient was adopted by all philosophers, and employed in their calculations, until M. Rudberg, a few years since, showed its inexactitude. By a series of experiments made with care, Rudberg sought to show that Gay Lussac's coefficient was too high, and that its true value was comprised between 0.364 and 0.365."

Regnault gives at length the experiments of Rudberg, and describes the instrument made use of; he then goes on to give a description of his own experiments, which were performed in four different methods, the first of which was similar to Rudberg's.

By the first method, the dilatation of air between 0° and 100° Cent. is shown to be—

1	. .	1.36556	8	. .	1.36634
2	. .	1.36626	9	. .	1.36689
3	. .	1.36659	10	. .	1.36610
4	. .	1.36579	11	. .	1.36671
5	. .	1.36625	12	. .	1.36591
6	. .	1.36549	13	. .	1.36641
7	. .	1.36673	14	. .	1.36673

"The mean furnished by these fourteen experiments is 1.36623. The difference between the two extreme numbers is 0.00140, that is to say, a little more than $\frac{1}{700}$ of the quantity that was measured. The numbers furnished by these fourteen experiments are all higher than the mean, 1.3646, that Rudberg obtained in his experiments, made by a similar process. I believe that this difference arises from the fact, that in the experiments of Rudberg aspiration of the external air took place; and it appears to me difficult to operate after his manner without this cause of error presenting itself, and it is evident that it must have escaped his attention, for he does not speak of it.

"The errors produced by this aspiration become more and more sensible, as the volume of air operated on is smaller. It was some time before I succeeded in avoiding this aspiration, and I am persuaded that it exercised a sensible influence upon my first experiments, making the numbers too small; and what confirms me in this opinion is, that from the moment that the aspiration became impossible, I never obtained a number lower than 1.3658."

In these experiments, it was necessary in one step of the process, to immerse a glass tube having a bulb at one extremity into

mercury, but the mercury not coming completely in contact with the tube, there would be a small portion of air adhering to it, and as the mercury would rise in the tube, the air would enter with it, and this is what Regnault here calls the *phenomenon of aspiration;* this he avoided by surrounding that part of the tube that was immersed in the mercury with tinfoil, and also by pouring a quantity of concentrated sulphuric acid upon the surface of the mercury.

Second method: the results obtained by this method are—

1	. .	1.36629	10	. .	1.36695
2	. .	1.36645	11	. .	1.36633
3	. .	1.36593	12	. .	1.36708
4	. .	1.36610	13	. .	1.36650
5	. .	1.36585	14	. .	1.36615
6	. .	1.36590	15	. .	1.36594
7	. .	1.36615	16	. .	1.36660
8	. .	1.36591	17	. .	1.36666
9	. .	1.36708	18	. .	1.36614

The mean of these experiments is 1.36633.

Third method: the results by this method are—

1	. .	1.36688	7	. .	1.36649
2	. .	1.36688	8	. .	1.36672
3	. .	1.36612	9	. .	1.36714
4	. .	1.36643	10	. .	1.36714
5	. .	1.36651	11	. .	1.36730
6	. .	1.36626	12	. .	1.36747

The mean of these experiments is 1.36679.

Fourth method.

1	. .	1.36592	4	. .	1.36682
2	. .	1.36710	5	. .	1.36674
3	. .	1.36662	6	. .	1.36580

The mean of these is 1.3665.

The following are the results of the four different methods:

First series, ...	1.36623
Second series, ...	1.36633
Third series, ...	1.36679
Fourth series, ...	1.36650
Mean, ...	1.3665

"Thus I adopt for the coefficient of the dilatation of dry air, for every degree of the Centigrade scale between the two fixed points of the thermometer, 0.003665."*

To this part there is a note, which is as follows: "M. Babinet has remarked to me, that in adopting for the coefficient of the dilatation of the air 0.366666 . . . this coefficient is represented by the fraction $\frac{1}{2\,7}$, which is very convenient to employ in calculation."

"The former coefficient admitted for the dilatation of air being found inexact, it is evident that we cannot regard as demonstrated, that it is the same for all gases, and new experiments are necessary to decide whether this law be rigorously true or only approximative.

"I have made experiments upon nitrogen, hydrogen, oxide of carbon, carbonic acid, sulphurous acid, cyanogen, protoxide of nitrogen, hydrochloric acid and ammonia."

The following are the results of his experiments compared with air, dilatation between 0° and 100° Cent.

For air, . . .	mean of	50	experiments,	0.36650
" nitrogen, . .	"	3	" .	0.36682
" hydrogen, .	"	4	" .	0.36678
" oxide of carbon,	"	2	" .	0.36667
" carbonic acid, .	"	4	" .	0.36896
" cyanogen, .	"	2	" .	0.36821
" protoxide of nitrogen,	"	3	" .	0.36763
" hydrochloric acid,	"	2	" .	0.36812
" sulphurous acid,	"	3	" .	0.36696

Regnault has just published a second series of experiments, and the coefficients for some of the gases have been changed; the reason for it will be seen in what he says.

"In all the experiments that have been as yet described, the dilatation of the gases have been determined in an indirect manner. The increase of the elastic force of a certain volume of the gas under an elevation of temperature was measured, and from this the dilatation deduced, supposing Marriotte's law to be correct. But, one can object by saying that this law is not demonstrated to be absolutely exact even for air."

"To leave no doubt as regards this mechanical theory of gas, I have made a new series of experiments, by means of a process

* This would be for every degree of Fahrenheit's scale 0.002036.

that would enable me to measure directly the increase of the volume of the gas, (it being apparently under the same pressure at 0° and 100° Cent.) This method is evidently the only one that we can employ for gases that do not follow the law of Marriotte under slight differences of pressure."

The instrument used was founded on the principle of M. Pouillet, *pyromètre à air*, and the results afforded by this series of experiments are—

For hydrogen, ...	0.36613
" atmospheric air, ...	0.36706
" oxide of carbon, ...	0.36688
" carbonic acid, ...	0.37099
" protoxide of nitrogen, . . .	0.37195
" cyanogen, ...	0.38767
" sulphurous acid, ...	0.39028

"The atmospheric air has afforded a number a little higher than the mean of the preceding experiments; but the difference is insensible, and besides it may be caused by the air not following rigorously the law of Marriotte.

"The oxide of carbon has given the same number as in the former experiments.

"The coefficients for carbonic acid and protoxide of nitrogen are much higher than those determined by the former experiments, and this is no doubt owing to the fact that these gases do not follow the law of Marriotte, and that their volumes at 100° Cent., under the greater pressure to which it finds itself subjected at this temperature in the first experiments, are smaller than they ought to be according to this law."

As for the difference in the coefficients of cyanogen and sulphurous acid, Regnault has ascertained that it was owing to an error in the first experiments, the gases not being perfectly dry.

The dilatation of gases under different pressures, calculated after the change of the elastic force.—"Philosophers admit generally, that the dilatation of gases is constant between the same limit of temperature, whatever be the pressure to which they are subjected; consequently, that it is entirely independent of the original density of the gas. But it is difficult to cite the experiments upon which this law is founded. Many observers having obtained the same value for the coefficient of the dilatation of air under different barometrical pressures, have concluded

that it remains the same under all pressures. But the variations of the barometer in the same place are too small to permit us to come to so general a conclusion; it serves to prove only that very small variations of pressure have no sensible effect in changing the coefficient of the dilatation of air."

The results of the experiments of Regnault upon air under different pressures are—

Pressure expressed in decimal fractions, the ordinary pressure being considered 1.00000.	Dilatation.
0.1444	0.36482
0.2294	0.36513
0.3501	0.36542
0.4930	0.36587
0.4937	0.36572
1.0000	0.36650
2.2084	0.36760
2.2270	0.36800
2.8213	0.36897
4.8100	0.37091

"The preceding experiments demonstrate that the law admitted by philosophers, that is to say, that the air dilates the same fraction of its volume whatsoever its density, is not exact. The dilatation of air between the same limits of temperature increases as the density of the gas becomes greater, or in other terms, as its molecules approach one another."

The same is shown to be true for carbonic acid.

Pressure expressed in decimal fractions, the ordinary pressure being considered 1.0000.	Dilatation.
1.0000	0.36856
1.1879	0.36943
2.2976	0.37523
4.7318	0.38598

Conclusions.—"My experiments do not confirm the two fundamental laws of the theory of gases, admitted up to the present time by all philosophers, viz.

"1. All gases dilate equally between the same limits of temperature.

"2. The dilatation of the same gas between the same limits of temperature is independent of its primitive density.

"Ought these laws now to be abolished? I do not think so. I believe that these laws, as well as all those that belong to the gases, such as the law of volumes, &c., are true to *a certain*

limit; that is to say, that they approach so much the nearer to results of observation, as the gas is examined in a greater state of dilatation."

"The laws apply to a perfect gaseous state, to which the gases approach more or less, according to their chemical nature; according to the temperature at which we consider them, which can be more or less distant from each of those points where there is a change of condition; finally and above all, according as they are more or less compressed."

ART. X.—*Description of the Bones of a New Fossil Animal of the Order Edentata;* by R. HARLAN, M. D., F. L. S., &c.—with three plates.*

Order EDENTATA.

Genus *Orycterotherium.*

O. MISSOURIENSE, *nob.*

THERE is now exhibiting at the Masonic Hall, in Philadelphia, one of the most extensive and remarkable collections of fossil bones of extinct species of mammals which have hitherto been brought to light in this country, a gratification for which our scientific community will acknowledge themselves indebted to the perseverance of the enterprising proprietor, Mr. Albert Koch of St. Louis, Missouri. This collection consists mainly, of the largest skeleton of an aged mastodon hitherto disinterred in America, nearly complete. The proprietor not possessing the advantage of anatomical knowledge, has committed some errors in the articulation of the bones, which, no doubt, his ulterior researches will enable him to rectify; among these errors may be noticed here, ten or more supernumerary vertebræ in the spinal column, some supernumerary ribs, and the first rib occupying the position of the clavicle, &c.

The upper portions of the skull had mouldered away, but this could be artificially replaced with accuracy—the collection including perfect skulls of the same species, and another specimen

* This memoir was originally read before the American Philosophical Society of Philadelphia, October 15th, 1841, and was reported in favor of publication in their Transactions. Uncontrollable circumstances having delayed the publication of its memoirs, leave was obtained for the withdrawal of the present paper.

having already been described by Professor Horner, in the Transactions of the American Philosophical Society for 1840. This skull was discovered in Crawford County, state of Ohio, and is in a prime state of preservation: the society has recently purchased it for the cabinet.

Mr. Koch's collection contains upwards of three hundred teeth of the mastodon, with numerous jaws, from the size of those of a calf, up to those of the immense skeleton.* There are also the remains of the ox, deer, elk, megatherium, and portions of the skeleton of *three individuals* of the *new genus* now under examination, judging from the relative size of the bones, some of which are duplicates. Of these individual skeletons, the following portions have been recognized: the os humeri of the largest, approaching that of the megatherium in size; the same bone of an individual about the size of the humerus of the *Megalonyx laqueatus;* and a third, younger and smaller individual, as is shown by portions of the clavicle, radius, &c. We have then, more or less perfectly preserved, two os humeri, two tibiæ, two portions of the radius, two portions of the clavicle; portions of several ribs; the cubitus, or ulnar bone, nearly perfect; sixteen loose specimens of teeth, and eight others in their sockets; two fragments of lower jaw, two fragments of upper jaw; five ungueal phalanges; the greater portion of a sternum, consisting of four pieces naturally articulated, and two other separate pieces; the major portion of the pelvis, &c. Numerous vertebræ were disinterred at the same time and place, and in the original notice of these remains were confounded with the skeleton of the new genus, but which subsequent observation referred to the skeleton of some phytivorous quadruped.

Not any of these bones are actually petrified, but having been imbedded in clay, are generally neatly preserved, and are very light and friable; the specimens having been coated with glue, it has given to them a blackish-brown color, but such as have lost this coating remain of a yellowish-clay or ochraceous color on the surface, and of a dead white color within. On subjecting portions of these bones to the action of muriatic acid solution, they were found to contain not the least vestige of animal matter.

* Among the most interesting specimens are two mastodon skulls, one adult, the other young, both of which have the tusks *in situ*, and which closely resemble the same parts in the elephant.

Geological position.—We copy Mr. K.'s description of the location of these bones, which he exhumed with much labor and expense, about two years since. The bones were found by the proprietor near the shores of the "Pomme de terre" or "Big-bone River," a tributary of the Osage River, in Benton County, state of Missouri, lat. 40°, long. 18°.

"There is every reason to suppose that the 'Pomme de terre' at some former period was a large magnificent river, from one half to three fourths of a mile in width, and that its waters then washed the high rocky bluffs on either side, where the marks of the waves are still perfectly plain: they present a similar appearance to that of the Missouri and Mississippi. Since the deposit of these bones, the bed of the 'Pomme de terre' has received several different strata, which occur as follows:

"Up to the time of the destruction of these animals, the original stratum forming the bed of the river consisted of quicksand. On the surface of this, and partly mixed with it, the bones were found. The next stratum is a brown alluvial soil, three or four feet in thickness; this contained and enveloped the bones. This stratum was mixed with a great quantity of vegetable matter, generally in a fine state of preservation; and what is still more surprising, all these vegetable remains are tropical, or of very low southern latitudes. They consist of large quantities of cypress burs, wood, and bark; a great deal of tropical cane, and tropical swamp moss; several stumps of trees, resembling logwood. Even the greater part of the flower of the Strelitzia class, which when buried was not full blown, was discovered imbedded in this layer; also several stems of palmetto leaves, one in which the fibres were nearly perfect. This stratum also contained iron ore.

"Next came a stratum of blue clay three feet in thickness; then succeeded one of gravel, from nine to eighteen inches in thickness, so densely compressed as to resemble pudding-stone. Then occurred a layer of light blue clay from three to four feet thick. On this, was another stratum of gravel, of similar thickness and appearance to the one first mentioned. This was succeeded by a layer of yellowish clay, from two to three feet thick; over this a third layer of gravel, of the same appearance and thickness; and lastly, the present earth's surface or soil, consisting of brownish clay mingled with a few pebbles, and covered with large oaks,

maples and elms, as nearly as could be ascertained, from eighty to one hundred years old. In the centre of the above named deposit was a large spring, which appeared to rise from the bowels of the earth, as it was never affected by the severest rain or the severest drought."

Description.—Teeth: these consist of sixteen loose specimens, together with eight others still remaining in their sockets, there being two fragments of the inferior maxilla, one containing three teeth, the other two, *in situ;* also two fragments of the upper maxilla, one with two, the other with one tooth, *in situ.*

In form these organs bear a general, and in structure a close resemblance to the teeth of the *Megalonyx laqueatus*, (vide Medico-Physical Researches, p. 319,) a new species of fossil animal from the valley of the Mississippi, which I described in 1831.* In most of the specimens now under consideration, the teeth are marked on one or both sides by longitudinal deep grooves, dividing the crown into two irregular shaped surfaces slightly depressed in the centre; vid. Pl. I, figs. 1 and 3. Exceptions are found to this form of the teeth in some of the molars, which are merely flattened cylinders, Pl. I, fig. 7; also in the anterior or tusk-like molars, which resemble elongated, gently curved cylinders, slightly compressed on the inner surface of the shaft, Pl. I, fig. 5. These teeth, like those of the Bradypus, the Megalonyx, &c. consist of concentric cylinders of bone, ivory, and of pars petrosa. They are very irregular in relative size and position in the jaws, their crowns being transverse, oblique, or longitudinal, as regards their position in the alveoles; vid. Pl. III, figs. 1, 2 and 3. The jaws may have contained from six to seven teeth in each side of each jaw, as one fragment of the inferior maxilla, a portion of the anterior part four and a half inches long, contains three teeth *in situ*, and which, from the thickness of the bone, must have been more than double this length; Pl. III, fig. 1. These teeth are above three and a half inches long, their crowns being one inch two tenths, by eight tenths. They are generally more or less curved and project from the base of the lower jaw upwards and forewards, this direction being reversed in those of the upper jaw. The ultimate molar

* Professor Owen has since proposed a new generic name for this fossil quadruped, the *Mylodon Harlani*. See some further observations on this animal in Vol. XLIII, p. 141, of this Journal.

Lithog. d'F. Bowen, 94 Walnut St. Philada.

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in the lower jaw, like the same in the *Megalonyx laqueatus*, is about double the width of the others, and resembles somewhat two molars, united in the middle by a compressed bony ridge; Pl. I, fig. 1. It is about equal in length to the others, (the lower extremity is fractured in the present specimen,) and two inches in its long diameter, the crown varying from four to eight tenths in lateral diameter. The anterior or tusk-like molar is more than four inches in length, (vid. Pl. I, fig. 5.) There are two other specimens of tusks, belonging to a younger individual, rather shorter, more slender, and almost cylindrical in form.

Maxillæ.—We have observed in this collection only the four fragments above noticed. A portion of inferior jaw is three inches deep, and two inches thick; Plate III, figs. 1 and 4—a section of the anterior end. A fragment of upper jaw is much thinner; the crowns of the molars here scarcely project above the rim of the sockets. The alveoles are distinct, and the teeth are separated from each other from one and a half tenths to four tenths; Plate III, fig. 3. Plate III, fig. 2, a portion of lower jaw, and figs. 5 and 6, sections of anterior ends of figs. 2 and 3.

Os humeri.—There are two humeral bones in the collection, that now described being a portion of the skeleton of much greater dimensions than the individuals from which the other bones were obtained; in fact, the animal of which this humerus formed a part, must have approached in size the megatherium, judging from the massive structure of the bone, and from its length, being little more than six inches shorter than that of the megatherium.

This humerus belonged to the right side; its general form is that of the megatherium, and is yet more massive, presenting stronger marks for the attachment of its powerful muscles; it is destitute of the large foramen near the internal condyle, which distinguishes this bone in the megalonyx, orycteropus, and most other genera of this order. In the proportional width or transverse diameter of the condyloid crests, it resembles the same bone in the megatherium, which is likewise destitute of the foramen above noticed; in the last mentioned animal, this transverse diameter is one half the length of the humerus, this part being always greatly developed in such animals as possess great muscular power in their fore-arms and hands. Thus, in the orycteropus this diameter is three fifths of the length of the humerus;

in the rhinoceros it is one third, in the elephant one fourth; in the Ruminantia, on the contrary, these crests are but slightly developed. The deltoid protuberances of this humerus are much more strongly developed than those of the same bone in the megatherium; the internal protuberance is continued down the anterior surface of the shaft in a longitudinal crest, which is most elevated at its lower extremity, or sixty inches above the elbow joint. On the outer side of this elevation is a still bolder protuberance, whose free border curves outwards and backwards, and which, being opposed inferiorly and posteriorly by the crest of the external condyle, forms a groove or channel sufficiently capacious to receive the arm of a man; and running in a curve half around the bone, from the posterior to the anterior surface of the humerus, affords a lodgment for a large and powerful flexor muscle of the fore-arm. A shallow trace of this groove is observable in the humerus of the *Megalonyx laqueatus*, and is more forcibly developed in the chlamyphorus, (vid. Harlan's Medico-Physical Researches, p. 47.) The shaft of the humerus is slightly concave posteriorly, and somewhat depressed in its antero-posterior diameter.

The radio-humeral articulating surface is peculiar, consisting of two distinct facets, an exterior hemispherical head, projecting one and a half inches below the internal articular face, which is slightly concave in its transverse and convex in its antero-posterior diameter, exacting a corresponding peculiarity in the articulating surface of the ulna, rendering the structure and arrangement of this joint somewhat *sui generis*. There is a broad depression just above the condyles posteriorly, to accommodate the motions of the olecranon. The following are the dimensions of this humerus:

Total length of the bone, ...	20 inches.
Greatest diameter of the shaft, ...	5 "
Antero-posterior diameter of shaft, . . .	$3\frac{1}{5}$ "
Circumference of the shaft, ...	14 "
Diameter of the superior head, . . .	$7\frac{1}{2}$ "
Transverse diameter of the condyloid crests, .	11 "
Long diameter of the head, ...	$7\frac{1}{2}$ "
Transverse diameter of the radio-humeral articulating surfaces, ...	6 "

Vide Plate II, figs. 1 and 2—vertical view of its head.

The other humerus of the smaller individual measures eighteen inches in length, and is not so perfect at the condyles.

Cubitus.—This is a short, thick, and strong bone, the marks for the attachments of the muscles being equally pronounced as those on the humerus; the present bone however belonged to the opposite or left side, and to another and smaller individual. The most remarkable peculiarity of this cubitus consists in the size and form of the superior articulating surface, and of the olecranon process; the latter is broad and long, projecting upwards and nearly in a line with the axis of the bone, the superior anterior angle looking inwards, unlike the same parts in the megatherium and megalonyx. The radio-humeral articulating surface of this bone consists also of two distinct facets or surfaces; the internal is much the highest and largest, is concave in its longest or antero-posterior diameter, being nearly four and a half inches long by two and a half inches wide. The posterior portion of this surface is continued downwards and inwards, to form the internal, smaller, and more concave facet; this again is continued at its anterior border into an irregular triangular disk, horizontal to the shaft, for the accommodation of the proximal head of the radius. These irregular surfaces are neatly adapted to the corresponding surfaces of the humerus, and, together with the olecranon process, occupy about two thirds of the whole bone, as will be perceived by the measurements below. The upper line or rim of the shaft of this bone descends from the strongly pronounced coronoid process, and ascends from the posterior superior end of the olecranon, rendering the ulna somewhat triangular in form, diminishing in size towards the carpal joint; the shaft is strongly compressed and deeply scalloped its whole exterior side, with two deep depressions on its interior surface for the lodgment of its powerful muscles. The carpal articulating surface of this bone is nearly circular and somewhat convex; the disk for articulation with the superior head of the radius, is on the inner surface of the ulna; the larger oblong disk for the lower head of the radius is on the anterior portion of the ulna, so that the hand would remain in the state of pronation, still enjoying some liberty of motion. Vide Plate I, figs. 9 and 10—a view of the proximal extremity.

Dimensions of the cubitus:—total length, 16 inches; distance from the coronoid process to the inferior extremity of the bone,

$9\frac{8}{16}$ inches; from the coronoid process to the base of the cubit, $6\frac{7}{16}$ inches; smallest transverse diameter of the cubitus, $3\frac{7}{16}$ inches, and reduced in parts by muscular pressure from one and a half inches to half an inch in thickness. The carpal articulating surface is broken.

Radius.—There exists in this collection two specimens of this bone of different sizes, both with the distal extremity broken off and lost; the smaller of the two, which apparently was attached to the above described cubitus, is about 7 inches long and $2\frac{1}{2}$ inches thick, compressed in its antero-posterior direction, and strongly marked by grooves for muscular attachments; its proximal extremity is terminated by a glenoid cavity, which moves on the external condyle of the humerus, measuring $1\frac{1}{2}$ inches by $2\frac{1}{2}$ inches, and is in continuance with a protuberance on the inner border, with a convex surface, articulating with the disk on the outer and superior portion of the ulna. The larger radius is nearly three inches thick, and eight inches long from the fractured end. The inferior articulating surface being lost in both specimens, we can offer no views of its form.

Carpus.—Five bones of the wrist have been preserved; two only of the upper row have been brought to articulate with each other. In general character, these carpal bones resemble those of the megatherium, in which, as in the present species, both bones of the fore-arm are articulated with the bones of the carpus.

Claws.—Four of the ultimate or ungueal phalanges, forming a continuous series, have been preserved; these are too small to have belonged to the large individual, their size being better adapted to the smaller one. These are much smaller than those of the megalonyx, and in general form approach nearest to the ungueal phalanges of the orycteropus. The upper border of the largest is convex, but less so than a similar bone in the megalonyx or megatherium, and rather more curved than that of the orycteropus; but the two next in size, like those of the last named animal, have the upper borders nearly straight, but in the fourth one, on the contrary, this border is in both rather concave. They all possess the bony tubercle, together with the foramina on its posterior inferior surface, like those of the megatherium and megalonyx; but they differ from these last named animals, in the absence of the bony sheath surrounding the base of the claws. The articulating surface in each is divided into two fa-

cets by a vertical central ridge, continued above into a projection of the superior posterior border of the phalange, as in the megalonyx and orycteropus. Vide Plate I, figs. 11, 12, 13, 14.

The dimensions of the claws are as follows:

Length of the longest, measured along its base, (the superior posterior part being destroyed,) . . .	$4\frac{1}{2}$ inches.
Next in size, similar measurement, . . .	$3\frac{3}{10}$ "
The same along its superior border, . .	$3\frac{1}{8}$ "
Third, " " " . . .	3 "
Fourth, " " " . . .	$2\frac{1}{2}$ "

Varying in depth from one to two inches; in breadth from one inch to half an inch.

Tibia.—Two nearly perfect have been preserved, one belonging to the middle sized, the other to the smallest individual, and the latter to the left leg; we shall describe the latter, more particularly as it articulates aptly with the astragalus, the only bone of the tarsus obtained. The tibia is a short thick bone, depressed in its shaft, and thinnest on its exterior border, and much enlarged at each end; the superior articulating surface presents a simple, shallow, ovoid disk, or glenoid cavity, oblong transversely. From all appearances, the fibula of this animal must have been small and insignificant, there being no mark for its attachment observable on the upper portion of either specimen; there exists only a doubtful depression on the internal malleolus of the smaller one; it could, however, have had no connection with the tarsus. The articulation of the knee is as simple in the present case as it is most complex in the megalonyx; but a plenary compensation is found in the lower ankle joint of this animal. At the anterior and inferior extremity of the tibia, between the malleolus internus and externus, nearer to the internal, just where the same part in the megalonyx and megatherium projects furthest downwards, there commences a socket $2\frac{3}{5}$ inches long, $2\frac{3}{5}$ inches broad, and $1\frac{1}{5}$ inches deep, which receives a corresponding globular or ovoid projection of the anterior superior portion of the astragalus. This cavity descends downwards and backwards to two thirds the thickness of the tibia at this part, when it unites with a transverse semilunar articulating surface, which is slightly concave near the external malleolus only; this joins a corresponding articulating surface on the astragalus, posterior to the globular or ovoid projection above described, on the outer side of

the base of which exists a foramen for the passage of vessels and nerves, large enough to admit the end of the little finger, into which, when at rest in an upright position, enters an angular projection formed at the outer and lower ridge of the socket. This extraordinary construction of the ankle joint is altogether proper to the animal under consideration, and which, whilst it affords the strongest possible movable articulation, would necessarily impede the hinge-like motion, in most animals very free at this part; and is confined principally to a semicircular and horizontal motion, as in turning the toes outwards and inwards. Plate I, fig. 15. The following are the dimensions of the tibia:

	Smallest specimen.	Largest specimen.
Total length, ...	10 inches,	$10\frac{1}{2}$ inches.
Transverse diameter of the superior head,	$5\frac{3}{16}$ "	$6\frac{7}{16}$ "
" " inferior head,	5 "	$5\frac{1}{2}$ "
Circumference of middle of the shaft,	$9\frac{1}{2}$ "	$10\frac{7}{16}$ "

Astragalus.—This is a large, irregular shaped bone, extensively marked with articulating surfaces, five in number; two for the tibia above, one below for the os calcis, and two anteriorly for the other tarsal bones. The articulating surface for the os calcis admits of a rocking or semi-ginglimus motion, in some degree compensating for the absence of this motion in the ankle joint, in which respect it differs from its congeners. Vide Pl. I, fig. 16. The dimensions of the astragalus are—

Extreme breadth, ...	$5\frac{3}{16}$ inches.
Height from the base of the bone to the top of the ovoid protuberance, ...	$4\frac{1}{16}$ "
To the base of the bone, ...	$2\frac{6}{16}$ "

Clavicle.—There are two or three pieces of bone resembling portions of clavicle. The two pieces now under consideration appear to have belonged to the smaller individual, and are the sternal and scapular extremities, each about five inches in length, two inches wide, and one fourth of an inch thick; the bones are of a strong structure, having but a slight development of cancelli within. Vid. Plate I, figs. 17 and 18—a transverse section.

Ribs.—Of the several portions preserved, one only is nearly perfect; a considerable portion of this has been fractured, and lost, from the sternal extremity. This piece is nearly two feet long, two inches wide, and one inch thick; the distance from the centre of one spinal articulation to the other is four inches;

the ribs vary in this respect from two to four and a half inches, according to their relative positions on the spine. Their structure is well developed.

Sternum.—Very little if any portion of this bone has as yet been found, either of the megatherium or megalonyx. Of the present animal we have a sternal piece, consisting of four bones united, measuring collectively thirteen and a half inches. There is one piece wanting from the top or anterior part of the column; another, or others, at the lower or posterior end. The second or top piece of this specimen measures five inches wide, three in depth, and five inches and five tenths in length. A longitudinal groove or channel runs along the inner surface. The facets for articulation with the cartilages of the ribs, measure from two to two and a half inches. Plate II, figs. 3, 4 and 5—transverse sections.

Pelvis.—A portion of the posterior, upper and lateral parts of the ilium, with four sacral vertebræ, are preserved, all ossified together; the anterior vertebra only of this piece being united with the ilium, though the adjoining vertebra above had also been articulated with the bones of the pelvis. The sacral bones proper had all lost their inner faces by decay. This portion of the ilium belonged to the left side; the whole is of a firm and massive structure. Three and a half inches below the sacro-iliac juncture, is another facet for union with the ischium, three by two inches in size. Vid. Plate III; fig. 7, the external, and fig. 8, internal view; fig. 9, articulating surface of the sacrum.

The present type is made up of points uniting it with the Megatherium, Megalonyx, Orycteropus, and Chlamyphorus, together with developments *sui generis* sufficiently characteristic.

P. S. Since the above memoir was presented to the American Philosophical Society for publication, I have observed in the American Journal of Science and Arts, Vol. XLI, p. 136, a very interesting notice of a new locality of similar fossil bones, communicated by Dr. H. C. Perkins, of Newburyport, Mass. These remains were found by Mr. E. Young, in December, 1839, on the Walhamet or Multnomah River, a tributary of the Columbia or Oregon, in latitude 41° north, about twelve feet under the earth. A tooth and os humeri of the Orycterotherium Missouriense, were found in company with the remains of Bos and Ele-

phas. The crown of the tooth as represented in the Oregon specimen differs from the same in the present species, but may have been injured by friction or other accident. The animal to which these Oregon specimens belonged is not named by Mr. Perkins, but he very properly remarks their resemblance to those of the Megatheroid tribe.*

ART. XI.—*An Account of some new and rare Plants of North Carolina;* by M. A. CURTIS.

Hypericum Buckleii: low, suffruticose, diffusely branched from the base, the branches slightly angled; leaves cuneate-oblong or obovate, subsessile, glabrous, very obtuse, minutely punctate; flowers terminal, solitary, peduncled; bracts subulate, small, a little below the flower; sepals foliaceous, unequal, oval or obovate, obtuse, much smaller than the petals, 3-nerved at the base; stamens numerous, shorter than the petals; capsule conical, acuminate with the united styles, 3-celled.

Stem 8–12 inches high. Leaves 6–7 lines long, 3–4 broad, pale beneath. Plant resembles *Ascyrum Crux-Andreæ.*—Discovered in 1839 on Whiteside Mountain, Macon County, N. C. Found the past season (1842) on many other mountains of North Carolina, south of the French Broad River, and in Georgia by Mr. S. B. Buckley.

Thermopsis Caroliniana: simple, erect, glabrous; leaves long petioled; leaflets oval oblong, cuneate at base, obtuse above, glaucous and with a fine appressed pubescence underneath; stipules large, about equalling the petiole, semicordate, clasping; flowers in a long terminal raceme, short pedicelled, alternate; bracts ovate, longer than the pedicels, deciduous; calyx silky villous, the teeth short and subequal, the upper one truncate, the lower triangular acute; legume oblong linear, straight, erect, appressed to the rachis, densely villous, 10–12-seeded.

Stem 3–5 feet high, glabrous except in the raceme. Raceme 6–12 inches long; leaflets 2½–3 inches long, about 15 lines wide,

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* Dr. Perkins proposed to name his specimen, if it proved to be a new animal, *Orycterotherium Oregonensis.* We have casts of the os humeri from both Dr. Perkins and Dr. Harlan, and can discriminate no difference; they must have belonged to the same animal.—EDS.

nearly equal; stipules 1½–2 inches long; legume about 2 inches, corolla (yellow) 8–10 lines long. Flowers May–July.—Discovered in 1839 near Pigeon River, Haywood County, and on the Hiwassee, Cherokee County, N. C. Found in many other localities the past summer by Mr. Buckley.

Thermopsis fraxinifolia: slender, branching, geniculate, nearly glabrous; leaves petioled; leaflets oblong, cuneate, smooth above, glaucous and with slightly pubescent veins beneath; upper stipules lanceolate, much shorter than the petiole, the lower larger and ovate; raceme terminal, peduncled; flowers (yellow) mostly alternate, on slender pedicels which are longer than the calyx and the linear lanceolate persistent bracts; teeth of the calyx short, the upper one emarginate, the lower triangular; legumes linear, falcate, compressed, spreading, pubescent, shortly stipitate, about 10-seeded.—*Baptisia mollis*, Nutt., *Gen.*, and Torr. & Gray, *N. A. Flora, in part. B. fraxinifolia*, Nutt., *Mss.*, ex Torr. & Gray.

Stem 2–2½ feet high; leaflets 2–2½ inches long; petioles ¾ of an inch; legumes 3 inches, on pedicels more than ½ an inch long, the stipe shorter than the calyx; flower 8–9 lines long.—Table Mountain; also on mountains in Henderson and Macon Counties, N. C.: Mr. Buckley. This plant does not turn black in drying, but the flowers become whitish. It appears to be confined to the mountains. My own specimens are derived from the same locality with Nuttall's *Baptisia mollis*, and are doubtless the same species. The plant collected by Schweinitz, (vid. Torr. & Gray, *N. A. Flora*, I. 695,) is probably the *Baptisia mollis*, Michx., to which a portion of the description in the N. A. Flora is alone applicable.

Baptisia mollis, Michx.: diffusely branching, and the whole plant rather softly pubescent; leaves petioled; leaflets lance-oblong, cuneate; stipules foliaceous, lance-oblong or ovate, about equalling the petioles; racemes terminal, peduncled, rather crowded; flowers (yellow) alternate or geminate, on pedicels shorter than the calyx; bracts lanceolate, longer than the pedicels; upper segment of the calyx obtuse or emarginate, the others lance-subulate; legume oblong, turgid, shortly stipitate.

Stem 2–2½ feet high; leaflets 2–2½ inches long, petioles about ½–¾ of an inch; pedicels 2–3 lines; legumes about ¾ of an inch, few seeded; stipe shorter than the calyx. Flowers bright yellow,

which, as well as the rest of the plant, retain their color in drying. Flowers April–May.—This plant appears to be confined to the middle districts of country, and is doubtless the plant of Michaux. It has probably not been seen since Michaux's time, unless perhaps by Schweinitz, until it was detected last year (1841) by Dr. C. L. Hunter on the ridge of land which divides the forks of the Catawba in Lincoln County, and is but a few miles from Mecklenburg, where Michaux discovered it. I have the past season found it near my residence, (Hillsborough, N. C.) growing on rocky wooded hills.

This and the two preceding species are well worthy of cultivation.

Helianthus Dowellianus: nearly smooth, branched above; leaves triplinerved, rather thick, slightly dentate, on margined petioles, and with a short scattered pubescence; the lower ones opposite, large, broadly ovate, subcordate, obtuse, the upper alternate, oblong-ovate; peduncles long and slender; scales of the involucre lanceolate, acuminate, slightly ciliate, shorter than the disk, appressed; rays 12–15.

Stem 4–5 feet high; corymbose paniculate at the summit, the branches axillary, long and slender, with a scattered appressed pubescence, but like the leaves smooth to the feet. Limb of the lower leaves 7–8 inches long, 5–6 wide, strongly triplinerved; the petioles 3–4 inches long, connate at base; rays about an inch long; young achenia hairy at the summit and angles; pappus of two subulate awns; chaff of the receptacle smooth, entire.

Around Franklin, Macon County, N. C. Flowers Aug.–Sept.

Vaccinium ursinum: young branches ferruginously villous; leaves oblong lanceolate or lance-obovate, cuneate or obtuse at base, short petioled, ciliate, mucronate, sub-acuminate, pubescent, particularly when young, covered with resinous dots beneath; racemes loose, naked; flowers campanulate, on slender pedicels; teeth of the corolla short; stamens included; filaments dilated, hairy; anthers unawned; calyx with short triangular acute teeth, and with the ovary and raceme resinously dotted; fruit black, 10-seeded.—Fruit matures July and August.

Shrub 2–3 feet high, with spreading rather straight branches; leaves 2–3 inches long, 1–1½ wide; racemes from below the leaves at the summit of the previous year's wood; fruit globose, red and acid before maturity, slightly astringent and insipid

when ripe. Found on most or all the mountains south of Buncombe County, but no where north of it. The flowers I had not seen until supplied this season with fine specimens by Mr. Buckley. In fruit it bears some resemblance to *V. resinosum.* It is well known, where it grows, under the names of *Bear-berry, Buck-berry,* and *Bear-whortleberry.* Dr. Gray informs me that it is noticed by Michaux, in his Mss. journal, under the name of "*Vaccinium d'Ours,*" but I cannot discover that it is described in his Flora. He probably found it in fruit, and included it under *V. resinosum.* But this cannot belong to the section with urceolate corollas.

Lindernia saxicola: stems numerous, erect, branching, glabrous; leaves opposite, ovate or oblong, obtuse, entire, the lowest attenuate at base, the upper clasping; peduncles strong, much longer than the leaves, refracted in fruit; corol four times the length of the calyx, the tube gradually enlarging from the base; segments of the calyx linear, subequal; capsule acute when young, terminated by the somewhat persistent style.

Stem 3–5 inches high; leaves rather distant; corol about 4 lines long, blue with lighter veins, the lower lip whitish with large blotches of blue.—Flowers in August.

On rocks in the Hiwassee River; also on rocks at Tolula Falls, Georgia.—Mr. Buckley.

Juncus cylindricus: stem compressed, leafy; leaves plane, smooth; panicle cymose, decompound; spikes cylindrical, many flowered, compact, mostly pedicellate; sepals 6, the exterior lanceolate, very acute, carinate, narrowly margined, the interior a little longer, oblong, obtuse, with a broad membranaceous margin; stamens 3, shorter than the obtusely trigonous capsule; anthers tawny.

This may prove a variety of *J. aristulatus,* Mich.; but I am inclined to consider it distinct. The cymes are small and but little expanding; stem 2–3 feet high; spikes 30–40 flowered; florets 3-angled by the strongly keeled outer sepals; capsule retuse, a little shorter than the sepals; style hardly perceptible; stigmas 3.—Lincolnton, N. C.

Muhlenbergia filipes: cespitose, glabrous; leaves very long, involute; panicle large, diffuse, capillary, not wholly exsert; pedicels long; calyx much smaller than the corolla, unequal, both valves long awned; corolla with the lower valve three-awned, the

lateral awns short, the intermediate very long, the interior valve short awned.

Stems 2–2½ feet high, forming large tussocks; leaves overtopping the culm, subulately attenuate; pedicels 1–1½ inch long; awns of the calyx unequal, the longest about two thirds the length of the awn of the corolla, which is an inch or more long; the lateral awns of the corolla are merely the disengaged sides of the summit of the valve. Habitat, sandy soil of the sea islands of North Carolina; also East Florida.—Flowers last of September–October.

This may be a variety of *M. Polypogon*, Trin.; if so, there are probably no characters by which either may be separated from *M. capillaris*, Trin.

Carex Mitchelliana: spikes in threes, peduncled, somewhat distant, oblong, subnutant; terminal spike staminate at the base and summit; the lowest peduncle scarcely sheathed; capsule ovate, acute, glabrous; scales oblong, the lowest with a long cusp much exceeding the fruit, the upper about equalling it. Habitat, wet places, Chatham County.—Flowers in May.

Stem 18 inches high, slender, acutely triquetrous, smooth, except at the summit; leaves shorter than the culm, smooth, except toward the tip; terminal spike staminate more than half its length, and a few staminate flowers at the summit as in the other spikes; spikes about an inch long, not very crowded; scales of the fruit at the base of the spike with a subulate point about a third longer than the capsule, but diminishing in length toward the summit, where they are about equal to and sometimes shorter than the capsule.

This is a distigmatic species and belongs to the same group with *C. crinita*.—Discovered in 1835.

Carex glaucescens, β. androgyna: spikes 4–5, erect, large, cylindrical, terminal one androgynous, (staminate at base,) the rest fertile.

Stem rigid, 3–4 feet high; spikes thick, 2½–3 inches long. This is an autumnal variety, flowering in October, and much the handsomest species I am acquainted with. Habitat, wet ground, Wilmington, N. C.

This species appears to be quite polymorphous. In its common form it has but one staminate spike; but it sometimes has two or more, and is then *C. verrucosa*, Ell.

Hillsborough, N. C., Nov. 1, 1842.

ART. XII.—*Selections from the Scientific Correspondence of* CADWALLADER COLDEN *with Gronovius, Linnæus, Collinson, and other Naturalists;* arranged by ASA GRAY, M. D.

DR. COLDEN, one of the earliest and most distinguished cultivators of science in North America, maintained, as is well known, an active correspondence with many of the most eminent men both in Europe and this country, on medical, philosophical, and scientific subjects, devoting to these pursuits the intervals of his public duties, as surveyor-general and member of the council, and, at a later period, as lieutenant and acting governor of the province of New York. Some of his letters and other papers on mathematical and philosophical subjects, in which he was particularly skilled, have been given to the public.* But, so far as I am aware, no part of his botanical correspondence has yet been published, excepting his two letters to Linnæus, which are included in the agreeable volumes edited by Sir James E. Smith.† Supposing that other botanical papers of equal interest might be brought to light, I availed myself of the permission kindly accorded me by David C. Colden, Esq. of New York, to examine the voluminous correspondence of his celebrated ancestor, and to select some portions for publication. I trust that these contributions to the early history of science in this country, will not be deemed inappropriate to the pages of the American Journal of Science.

Although Dr. Colden had acquired the rudiments of botany, as taught in the University of Edinburgh at the beginning of the last century, yet he paid little attention to the subject for twenty or thirty years after his arrival in this country. But having casually obtained some of the earlier writings of Linnæus shortly after their appearance, he zealously engaged in the examination of the plants around him, according to the new system, and soon established a correspondence with Gronovius of Leyden, Peter

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* Vide *American Medical and Philosophical Register*, Vol. I, (1810,) which contains a spirited biographical memoir of Gov. Colden, from the pen of Dr. Francis; and *Sparks, Works of Benjamin Franklin*, Vol. VI, (1838,) *passim*.

† *A Selection of the Correspondence of Linnæus and other Naturalists, from the original manuscripts*, Vol. II, p. 451–458.

Collinson of London, and, a few years later, with the great reformer of natural history himself.

I have not been able to find any draft of Dr. Colden's first letter to Gronovius, (acknowledged in the subjoined epistle from that botanist,) in which he transmitted a detailed description of the plants growing around his country residence at Coldenham, (near Newburgh,) New York. This communication was afterwards sent by Gronovius to Linnæus, who, in the year 1749, published the first part of it (to the end of class *Polyandria*) in the Transactions of the Royal Society of Science at Upsal, under the title of *Plantæ Coldenghamiæ, in provincia Noveboracensi Americes sponte crescentes, quas ad methodum* Cl. Linnæi *sexualem, anno* 1742, *&c. observavit et descripsit* Conwallader Colden. This early attempt at the systematic arrangement of our plants is really an extraordinary performance, considering the circumstances under which it was prepared, and fully merits the praise which Linnæus and Gronovius bestowed upon it. The plants are described with great accuracy and scientific skill; their medicinal uses indicated, and several new genera are proposed, although not named. The *magnitude* of this treatise is, however, greatly and strangely overstated by Colden's successor, the late occupant of the gubernatorial chair of New York; for instead of *filling two quarto volumes*, as Governor Seward gravely states, in his elaborate *Introduction* to the Natural History of New York, it actually occupies about twenty pages!

To Dr. Colden's first letter, Gronovius replies (in English) as follows:

Gronovius to Dr. Colden.*

Leyden, August 6th, 1743.

Sir—The 29th of July I was favored with your kind letter of the 28th of March, 1743, which came to my hand (by the care of Doctor du' Bois,) by the Rev. Mr. Dorsius; who told me, that in a short time he was resolved to go back to Pennsylvania; wherefore I shall answer to your letter as much as the time will permit.

Mr. Clifford, one of the richest merchants at Amsterdam, has printed his *Hortus* at his own expense, and doth not sell any copy, but is very liberal in making a present of it; so that if you

* The orthography in these letters has been corrected and modernized, but the phraseology is strictly adhered to.

can collect some seeds for him, I do not doubt he will present you with a copy of it. Sometimes it is to be met with in a public auction, where it commonly goes for about twenty five guilders.

The second part of the *Flora Virg.* is printed, of which I take the liberty to send to you a copy, besides a copy of the first part. But as you make no mention in your letter of some other books of Linnæus, besides his *Characters*, I take the liberty to present you with some other books, which you will find of great use to you, viz. with a new edition of his *Fundamenta Botanica*, in which you will find a great thesary of learned observations; but this book must be read over and over, and then you will easily perceive the laws which must be observed in making the characters, particularly about the *partes fructificationis*, where the *numerus, figura, proportio, et situs*, always must be observed.

I am infinitely obliged to you for the plants and characters you are so kind as to communicate to me. At present I am very much taken up with the public affairs, and the short staying of the Rev. Mr. Dorsius is the cause that I cannot examine your characters. What [can] I hope to do when our 20,000 men for assistance of the queen of Hungary, are marching, being about these affairs not only the seven provinces, but the towns themselves divided in their opinions! However there is great hope to a general agreement.

However, in reading now and then for a moment your characters, I am (without any flattery) surprised how you, in such a short time, could have such ideas of Linnæus's way in making up the *notas characteristicas*. Linnæus hath promised to give out one time or another the *Philosophia Botanica*, which should only consist of an explication upon the *aphorismi* of his *Fundamenta;* so that this book should be as a standard. A part of it is printed in the *Critica Botanica*, wherein certainly you shall find exceeding good remarks; wherefore I take the liberty to present to you my own copy of that book, which I have more than fifty times read over and over. I do not doubt you shall find by reading nicely the *Fundamenta*, the preface to the *Charters*, and this *Critica Botanica*, you may easily make yourself master of Linnæus's way. For myself, I assure you, I cannot give to you better directions. But if it is that you have still some scruples, I shall always be ready to answer them as much as I can.

Dr. Linnæus is not only content with his botany, but he extends his industry to all parts of natural history, and has brought me over to it, particularly persuaded by the power of my family, and I myself, too, in the government of this town, and by that way now and then departs to the states of Holland and other colleges. I get by our men of war an immense collection of natural things, of which my chiefest delight is in the *Lapides* and *Testacea*, that is, the *conchæ et cochleæ*.

I have tried *juxta Linnæi Fundamenta Botanica*, to give names to all these things, and printed a catalogue of it in the year 1740; since which time my collection increased once larger. You shall infinitely oblige me if you meet some of these things, to communicate them to me; particularly the *cochleæ* and *conchæ* of your country. I don't doubt there is in your country a good variety of snails, (as well at land as to the rivers,) whose *cochleæ* shells are different in shape and colors.

Now, sir, once more, I shall acknowledge myself very much obliged to you for your letter and characters, assuring that I shall always be glad to see your letters, to which I shall always answer upon [the] spot. Direct only to Mr. Sadelhof, merchant at Amsterdam, or to Mr. John Papin, junior, merchant at Rotterdam, with a direction to Dr. John Frederic Gronovius, Senateur de la Ville de Leyden, in Holland.

I am, dear sir, wishing you all health and prosperity, your most obedient servant, JOH. FRED. GRONOVIUS.

Leyden, October 3d, 1743.

SIR—The before-mentioned Mr. Dorsius hath told me that he was resolved in short time to go over to Pennsylvania, and promised me that he should call upon me and spend a night; whereupon I wrote these preceding lines, and made a packet of things mentioned there. To my great sorrow I did not hear any thing of that gentleman. In the mean time, I resolved to fix daily some hours to consider your characters. Once for all you must know that I [am] one who never will flatter any one; but with father Plinius *agnoscere per quem profecerim*. Indeed, I must confess, I hath [obtained] a great deal [of] light in your characters, about some plants mentioned in the *Flora Virginica*.

I take the liberty to send you my remarks, which have been ready a month ago, when I hath made this packet ready, that at the approach of Dorsius I could give it immediately in his

hand, but alas! the fellow did not come; so that I was a whole month disappointed, having been very sorroy that I going to bed saw the packet lying in my room, and the next morning coming in my room, I saw it still lying there; till it happened that Mr. Otto, a gentleman from .... advised me of his going to England, who promised me to give it there in good hands.

I remain, dear sir, your most obedient servant,

JOH. FRED. GRONOVIUS.

GRONOVIUS TO DR. COLDEN.

Leyden, April 3d, 1744.

DEAR SIR—A month ago, I hath the pleasure that my particular friend, Mr. Canwan from St. Christopher, going to London, did me the favor, to take a small packet with him, directed to you, in order to look there for an occasion to send it to you. In the same packet you will find an answer to your characters, the *Fundamenta Botanica* of Linnæus, and his *Critica*, with the second part of the *Flora Virginica*, and my *Index Supellectilis*.

Since I have found that your characters, of which you send me a specimen under number 19, is responding to the *Diervilla*, but I believe a quite different species. In the mean time I had an occasion to write to my good friend Linnæus, and to get an answer from him, of which I communicate to you some particulars.

"Literas 17 Sept. datas accepi: ex iis percepi placuisse summo arbitro novum creare in America Botanicum; si ille tam multa præstet per te ac Claytonus, erint plantæ Americanæ certiores quam Europææ; et nulla rerum vicissitudo plantas et hos botanicos unquam obliterabit.

"Videtur certe cl. Coldenus vir acuminatus et occulatissimus: hoc tamen video, quod si ipsas plantas non communicat nobiscum, minus utilis erit. Hoc vero si fecerit, erit systema sexuale, et plantarum caracteres, et differentiæ tales, quales tota Germania, ne quidem Europa unquam proculeabit. Jucundissimæ mihi fuerunt observationes ejus, quas mecum communicas. Ego ad singula respondes." * * * * [Here follow several notes and remarks upon Colden's characters.]

These are the remarks which Dr. Linnæus hath made upon some heads of your characters, about which I have wrote to him. To satisfy him more, I left a copy for him of your characters. At the next occasion, you shall see the *Oratio Linnæi de Tellu-*

ris habitabilis incrementis, and *Celsii oratio de mutationibus generationibusque in superficie corporum cœlestium contingunt.* In both of which you shall find curious observations.

I hath kept this letter with me with intention, as soon [as] the winter is gone, to send it to you. It is now the 26th of February, when our public trek-boats went the first time to Amsterdam in this winter, having had a mighty pleasant frost, only two days very severe, but no snow at all.

In the mean time, I discovered that my friend who hath the care of the before-mentioned packet, hath not done right with some other packets. I suspect the same accident may [have] happened to the packet directed to you, wherefore I send to you another copy of Linnæus's *Characters*, of the *Flora Virginica*, of the *Fundamenta Botanica*, and my *Index*, besides another copy of my remarks upon your characters. There was in the before-mentioned packet a letter to you, but having no copy of it, I hope you shall excuse me to write another. The *summa summarum* was, that I always shall be glad with your letters, and sincerely answer to them; the second part of my letter was that I am thinking and meditating for a natural system of the *cochleæ* and *conchæ;* so that I beg you will be so kind to send to me the *testæ* of the land and sea-snails which are common in your country; and if there are to be met some shells and oysters, you shall oblige me with a couple of each species.

If you have any thing for me, pray direct it to Messrs. Dan. and Bar. Van Zadelhoff, merchants at Amsterdam, by whose care you get this paper. I shall be glad to hear from you as soon as you have received these things. If I can be more of any use to you, pray command freely, your most obedient servant,

JOH. FRED. GRONOVIUS.

P. S. As you have seen that Dr. Linnæus hath desired a copy of all your characters, I have sent them to him. I hope by my next letter to you to have more of his considerations upon them, which I shall faithfully communicate to you.

The following letters exhibit Dr. Colden's just appreciation of the wide difference in character and object between an artificial and a natural classification. His proposition to arrange plants in natural groups or orders, in accordance with the *ensemble* of their

affinities, employing some strictly artificial scheme for the single purpose of arriving most readily at the name of an unknown plant, as well as his remarks upon the gradations from one class and genus to another which the natural system when discovered may be expected to present, will appear really surprising, when it is remembered that he could have read nothing upon the natural classification more modern than the *Fragmenta Methodi Naturalis*, appended by Linnæus to his *Classes Plantarum*, (1738.) These letters are copied from the original drafts preserved among the Colden papers.

Colden Vol # 3 p 83 Dec 1744?

DR. COLDEN TO GRONOVIUS.

[No date.]

DEAR SIR—Your favor of the 3d of April, which I did not receive until the 15th of November, has so far exceeded the fondest of my hopes, that you have thereby laid me under the strongest obligations. I was, and still am, so conscious of my want of knowledge in botany, that I with good reason apprehended it was not in my power to be of any use to you or Dr. Linnæus, both of you consummate in that science. I cannot cease to admire the unwearied diligence and surprising accuracy of Dr. Linnæus, in forming his characters of such a vast number of plants. But it is to you more immediately that we in America are indebted; and it was merely in gratitude for the benefit we in America have received from your labors, that I offered any little assistance that is in my power, and which you have now laid me under the strongest obligations to perform. I must therefore previously excuse an imputation of negligence, which I am afraid I shall hardly avoid, in not complying with all that you may justly expect from me. For, as I am in public employments, I am frequently, during the summer season, obliged to attend to them in the city, where I have neither leisure nor opportunity to examine plants. So it has happened to me these two last summers; and it was accidental that, in the summer before them, I had so much leisure to examine the plants growing near my house in the country, and to make the observations which I sent to you. I shall next summer endeavor to collect for you all the specimens which you desire; and when I meet with any other plants which I think deserve your observation, I shall send you specimens of them, together with my own observations. I thank you likewise for your present of Linnæus's *Characters*, his *Fun-*

damenta Botanica, his *Oratio de Telluris habitabilis incremento*, *Celsi Oratio*, and your *Flora Virginica*, *Index Supellectilis*; all which I have received safe. But those by Mr. Canwan I have heard nothing of.

When you write to Dr. Linnæus, pray offer my humble service to him, and assure him that I shall be very proud of receiving his commands, if in any thing I can serve him in this part of the world. Last summer I sent the characters of the *Actæa* and *Christophoriana baccifera* to my good friend Mr. Collinson of London, which I believe he was to communicate to Dr. Linnæus. It was from Mr. Collinson this spring that I learned with pleasure that you had received my letter; for I began to suspect that it was either lost, or not worth your notice.

Since you have given me so much encouragement to propose my doubts on the Linnæan system, I shall take the liberty to make them as they occur, without further ceremony, trusting to the indulgence which you have already so fully shown me; though what I now presume to make goes even to the general distribution of his system into *Hermaphroditi*, *Monœcia*, *Diœcia*, and *Polygamia*. For if there be species of plants which are evidently of the same genus, and yet according to this system must be referred to different classes, you must certainly allow it to be a fault in the system. What has given occasion to me for this doubt, is what I wrote to you before, that I had observed some particular plants of the *Clematis* that carried only male flowers, while others of the same species bore all hermaphrodite flowers. There is no doubt of these plants being of the same species, because they agreed in every thing in the leaf, in the stem, &c., so that not the least distinction could be observed, except that the flowers in one were all male, in the other all hermaphrodite; neither did I observe any other plant of the same genus that had female flowers. The next is what I likewise observed to you before of the *Sagittaria*, that the species which I observed was evidently distinguished into male and female plants. You may assure yourself it is not an *Alisma*, according to Linnæus's character of this genus, nor no new genus; but that in every thing it agrees with the character of the *Sagittaria*, excepting that the flowers are male and female in different plants of the same species. Now I shall mention a third plant, a specimen of which I sent you, though without the flowers, no. 198,

and which you tell me is the *Myrica foliis oblongis alternatim sinuatis*, Fl. Virg. p. 192.* According to Linnæus's character of the Myrica, it is of the *Diœcia* class; I can assure you that this species is of the *Monœcia*, and carries male and female catkins on the same plant. * * * I cannot think that I have accidentally fallen upon all the exceptions of this kind that are to be found. * * * In vegetables it is necessary that the males be in greater number than the females, in order to make sure of their impregnation; and therefore I do not think it against nature to have in the same species one plant with all male flowers, and another with hermaphrodite flowers.

One reason for Dr. Linnæus's establishing so many classes, (I suppose,) is to avoid as much as possible any of them from being too much crowded. This, I think, may be done, by dividing, as Mr. Ray and others have done, plants into trees and herbs. This is a distinction that all mankind make, and therefore I cannot doubt of its being a natural distinction; and certainly an obvious natural distinction is to be preferred to one more obscure. As to my part, if two plants, one a tree and the other an herb, should happen to agree in every part of the fructification, yet I could not persuade myself to think them of the same kind; there is something so very different in the whole formation and constitution between a tree and an herb. I know it is objected that there is no certain criterion to distinguish them, that any criterion hitherto given by botanists will agree to some herbs: but this objection I do not think sufficient, for it may be only a proof of our want of knowledge in giving the proper criterion, not that the distinction is not real. If this objection should hold, it may go further, even to destroy all distinction between vegetables and animals; because I know no criterion to distinguish animals from vegetables but what leaves room to doubt to which of them some species belong: witness the polypus, which has been the subject of late observation. Indeed, my opinion is, that the natural gradation from the lowest class to the highest is by such small and imperceptible steps, that it is very difficult to distinguish even the next step, either upwards or downwards; though at some distance the distinction be very remarkable. *For this reason, any system in botany would give me a strong prejudice in its favor, where there appears such a*

* The *Comptonia asplenifolia* of Gærtner.—A. G.

gradation from one class to another, and from one genus to another, through the several species, that the step from one to the other becomes almost imperceptible. Whenever this system shall be discovered, I shall conclude it to be the natural system.

Give me leave to make another objection to the Doctor's system; for it is not with any view to depreciate so worthy and great a performance, but that I wish to have it as perfect as possible; and I hope he will live to make it such, as I know no man so capable of doing it. It is this, that the distinction of the *Syngenesia* class, according to the male, female, hermaphrodite, and neutral flowers is so very nice, and in many cases requires such a clear sight, and is apt to run the observers into confusion; which, by comparing the first and second editions of his characters, the Doctor himself has not been able to avoid in some instances. Add to this what I have before observed of the Clematis, Sagittaria, and Myrica, and we may have room to suspect that it does not truly and naturally distinguish the genera, but that the same species are subject to variations with respect to these. But it is time for me to stop. *Ne sutor ultra crepidam.* However, I must again mention what I before hinted to you of the *Gynandria Diandria*, that I still think that one distinguishing part of this natural class, among other parts of their character is, that the stamina or antheræ are affixed to the nectarium in some shape or other. This I have observed in all the species that I have had an opportunity to examine; which are indeed so very few that I can rely no more upon it than to recommend it to your examination; for I could not, after reading Linnæus's description of this kind of flower, discover any other stamina than what I take to be such. Since I wrote my former [letter], I examined the *Cypripedium;* there in the hollow of the nectarium, this down or fine hairs appear, and if I be not mistaken, the antheræ are affixed not on their summit or top, as usual, but to the sides of the filaments below their summits. Two stamina seem not sufficient to me to impregnate the great quantity of seed contained in the capsule. Nature every where else seems careful to make sure work, even by profusion. You, who have the advantage of botanical gardens, may soon be satisfied whether there be any real ground for my conjecture.

I have long wished to see Dr. Linnæus's *Philosophia Botanica*, that is, ever since I saw the name of it mentioned, and was a

little acquainted with his works. But when you wish in vain, it is needless for me to add my wishes. The reasons the Doctor gives you for suppressing that book, in my opinion, should not prevail with a good man in depriving mankind of a general benefit. * * *

The benefit of oil in the cure of the bite of the Rattlesnake has been confirmed in several instances in this country, and even that hog's lard is effectual; and of consequence it is probable that all oily things are. It has long been observed in this country, that hogs were never hurt by the bite of the Rattlesnake, or by any viper; though all our other cattle at some time or other have been. This I attribute to the natural defence they have by their fat, through which the teeth of the viper cannot penetrate, without giving the remedy at the same time. You know how dangerous it is to trust to experiments, unless they be performed with all requisite precaution; but I can assure you that I would rather trust to oil or hog's lard than to the famous *Polygala*, or Rattlesnake-root, or to any other medicine that I have heard of; because the beneficial use of this comes better confirmed to me than that of the Rattlesnake-root or any other. We chiefly trust to the warm external application.

As you seem to be pleased with my communicating the use of any plants discovered in this country, I shall tell you what I learned of the use of the *Hamamelis* from a minister of the Church of England who officiates among the Mohawk Indians. He saw an almost total blindness, occasioned by a blow, cured by receiving the warm steam of a decoction of the bark of this plant through a funnel upon the place. This was done by direction of a Mohawk Indian, after other means had for a considerable time proved ineffectual. * * * Dr. Linnæus is right in observing that I had mistaken the gemma of this shrub for the involucrum, in the description I gave of it.*

I have not yet been able to see the fruit of no. 131, but I cannot doubt of its belonging to the class of the *Tetradynamia*, for

* Respecting Colden's description of this plant, Linnæus writes to Gronovius: "Quod vocat *Involucrum* est *Gemma*. * * Nectaria egregie describit, quæ videre nequivisti: fructus a te missus docet esse capsulam duram, nec nucem." Nevertheless, Colden's detailed character of the genus (*Plantæ Coldenghamiæ*, no. 18.) is not only perfectly correct, but is adopted by Linnæus in his *Genera Plantarum*, &c., where the character commences with "*Involucrum triphyllum, triflorum*," etc.—A. G.

besides its agreeing in all its parts of the flower with the characters of that class, it agrees likewise in taste, having nearly the same with that of the *Nasturtium*. The Mohawk Indians told me that when they were quite faint with travel and fasting, if they can come at the roots of this plant to eat, they are refreshed, and their spirits restored wonderfully. Add to the character I gave what follows: *Radix longa, teres, repens, prominentiis plurimis angulosis.**

What I wrote to you of the species of the *Zea semine nudo*, I believe is entirely a mistake; for having some of these seeds, the plants which came from them produced seeds covered with a hard skin, as the other sorts are. I suspect some artifice was used to deceive me. Whatever Tournefort may say, I cannot doubt of these being distinct species of the Maize, notwithstanding that they cannot be distinguished either by the leaf or flower. But we that are well acquainted with the seed can distinguish the species, though it be very difficult to convey that distinction by words only to others. Sow the several species in the same soil and at the same time, they will come to ripeness at very different seasons in the year; and this property they never change, unless when sown together so that they bastardize.

This brings to my mind a thought which I have entertained, viz. that we have in America very few if any species of plants or animals entirely the same with those in Europe, except such as have been brought from thence: although some species are so nearly alike that it is difficult to describe the difference by words; though it be manifest to a curious observer.

If I, who understand so little of botany, were permitted to advise, I should propose the plants to be collected into their natural order or classes without regard to any system; after which I would make a system by which the same plants should be disposed according to the rules of that system, with the view only to assist learners or the ignorant to discover the proper name or place of each plant. And in this I would have no regard to the natural system, but even divide and separate the species of the same genus into different classes, if the rules of my system required it; for I would have this system to be looked on as nothing else but as an index to discover the plant one desires to find

* The plant is doubtless the *Dentaria diphylla*.—A. G.

in its proper place; and therefore I should prefer a system that serves best to this purpose, though it should no way agree with the natural system. By this means many disputes would be avoided among botanists, and the system would be more beneficial to learners: for thereby they would have a double method of discovering any unknown plant; first from its natural conformity to some other known plant, and secondly, from some remarkable part of its character by which they are in the artificial system led to it.* I cannot forbear to wish that you would try this method in a new edition of your American plants, for my sake, and other unskillful philo-botanists in America.

I shall be obliged to you, if you will please to inform me of any new valuable works in medicine published with you. I have not the good fortune to have seen any thing in the materia medica that entirely pleases me.

You cannot expect much new in literature from this part of the world. I send with this a curious and new invention for warming a room with a small fire more effectually than can be done by a large fire in the common method, and is free of the inconveniencies which attend the Dutch and German stoves; because by this contrivance there is a continual supply of fresh warm air.† It may be particularly useful to you and Dr. Linnæus, by preserving your health while it keeps you warm at your studies. It is the invention of Mr. Benjamin Franklin, of Philadelphia, the printer of it, a very ingenious man. Experience confirms the benefit of it. * * *

I design to give our friend, Mr. Collinson of London, the trouble of conveying this to you; because we have not any ship at this time designed from this to Holland. He does me commonly the pleasure of writing to me twice in the year, at the seasons our ships commonly leave London, viz. in the end of February and beginning of September. When any ships from Amsterdam either go to New York or to Philadelphia, if your friends at Amsterdam please to direct your letters for me by the New York ships, to the care of Mr. Richard Nicholls, Postmaster in New

* This proposal to employ a natural system of classification, with an artificial anaylsis to facilitate its application, appears to correspond entirely with the methods now in use.—A. G.

† The pamphlet here referred to (printed in 1774) is reproduced in *Sparks, Works of Franklin*, Vol. 6.—A. G.

York, or by the Philadelphia ships to the care of Mr. Benjamin Franklin, Postmaster in Philadelphia, they will come safe to my hands.

Dr. Colden to Gronovius.

Coldengham, in New York, Oct. 29th, 1745.

Dear Sir—I answered yours of the 3d of April, 1744, near twelve months gone, directed to the care of our common friend Mr. Collinson of London; which he tells me he has carefully transmitted to you. This is destined to be sent likewise to his care, since he is pleased with having this trouble put upon him. I am so little acquainted with the merchants in this place who trade directly to Holland, that the ships are commonly gone before I hear any thing of them. Besides, most of these merchants and masters of vessels are very careless of any thing of which they have no prospect of any profit.

With this, I send you the characters of some more plants, which I observed this year, and some corrections or additions to what I before observed, some dried specimens, and some seeds. I have presumed, with that freedom which is I think allowed in all philosophical inquiries, to mention some further difficulties which arise to me in the Linnæan system; and though you may perhaps easily solve them by showing my ignorance in botany as a science, yet, as probably the same difficulties may occur to others, it may be of some use, by giving you an opportunity of clearing up this matter, to others less versant in that science. * * *

My opinion that we have no species of plants in America precisely the same with those of Europe, requires much more knowledge of the plants than I can pretend to in support of it. But at the same time it will be difficult to demonstrate that it is false; for certainly some species differ, in which it is difficult to show the difference in words. For a carpenter can know a chip of one kind of timber from another, and can distinguish a piece of American oak from the English. We, who are used to the woods, can distinguish the trees and their several species by the bark alone; and yet I believe the most able botanist would be puzzled to describe either the grain of the several kinds of timber, or the differences of the superficies of the bark, so as to enable a stranger to distinguish them without further assistance. As to animals, I have never seen any precisely the same.

I must still insist upon it that there are several species of maize, though there be no difference in the shape and proportion of the several parts; but there is a great difference in the bulk and stature of the several species, and of their time of coming to maturity, though planted in the same soil and at the same time. Neither can I conceive that there can be any variations of the same genus where there are not different species: because I think that the variations arise from the farina of one species impregnating another species. * * * * We are well assured that all the Indian nations as far north as New England, planted maize when the English and Dutch first discovered them; and it is for this reason that it has obtained the name of Indian corn among the English. The Indians towards the sea on the New England coast, planted a small yellow sort, early ripe; those in the northern parts of New York, a small white sort, likewise; the Indians in the southern parts of New York, in Jersey and Pennsylvania, a larger sort, of variety of colors, and ripe a month later than the former sorts; and in Virginia and South Carolina they planted maize which rises to a great height, but is late in coming to maturity. In the several parts of the country where only one species is planted, no variations are observed. Now, sir, I think it not probable that all these far distant nations should so soon have received maize from the Spaniards, and to have adapted the several species of it to their several climates; and from the Spaniards only could they have obtained it, if it was not a native of their own country. But I even doubt if maize was common in Spain before the discovery in America. Besides, we observe that the Indians very slowly receive any of our customs; notwithstanding that the English and Dutch have lived so long with them, no Indian nation has hitherto sowed wheat or any other of our grains. For these reasons, I conclude that maize is a native of America, and that we have different species of it from those of Asia. My negroes tell me that they have kinds of maize in Africa very different from any in this country; and I am lately told that the Turkey maize is a different species from any we have.

I have, in considering Dr. Linnæus's characters of plants, endeavored to form some criterion for myself, whereby to know, when any new plant offers itself, whether it be of a distinct genus of its own, or a species of some other genus already known;

and to judge which of the botanists have pursued the method most conforming to the natural in their system; for I find that they all sometimes differ widely in this respect. For example, Linnæus ranks under the genus of *Convallaria* and under *Lonicera*, plants which all other botanists referred to several different genera: indeed, I think the parts of fructification in many of them may be sufficiently distinguished. On the other hand, it is very difficult for me to distinguish the genera of many of the class of *Hexandria* which he makes distinct. Since there is no manner of concord as to this grand point in botany among the ablest botanists, I may be allowed to suspect that no sufficient criterion has yet been discovered.*

I must therefore conclude, that however unsystematical and artless Mr. Ray's taking in the *tota facies* of plants into his distinction of the genera may be thought, we cannot entirely throw it aside, till some better criterion be found than has hitherto been given by botanists. For, so far as I can judge from the characters of plants given by others, and what little observation I have made, the parts of fructification alone, that is, the outward shape and form, number of cells, &c. is not sufficient in every case to distinguish the genera. Neither are the general shape and form of the leaves, stalks, and roots, or their proportion to each other, sufficient to distinguish the species, but that sometimes some other circumstances are to be considered, as particularly that of the time in which they acquire maturity.

But notwithstanding these objections I make to Dr. Linnæus's system, they no way lessen the esteem and value I have for his works; for I am more obliged to him than to any botanist I have seen. It is wonderful with what exactness he has observed such a vast variety of plants; and it is as wonderful to me that he has made so few mistakes in this great performance. What objections I make to his system will almost equally affect all systems. We have not as yet sufficient knowledge to adapt our systems

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* Dr. Colden then proposes the faculty of hybridization as a test of this point, and considers those species which hybridize as belonging to the same genus, and those that cannot be made to hybridize to belong to different genera, and proceeds to defend this view by various illustrations drawn both from the vegetable and animal kingdoms. In his subsequent letter to Linnæus, (published in full by Smith in his *Correspondence*, *l. c.* Vol. II, p. 452,) he considers the same subject in a similar manner. It is hardly necessary to state, that he seems to confound proper varieties with hybrids.—A. G.

to nature; and therefore every systematic writer in every science would have forced nature to comply with his system, where he found that he could not bring his system to nature. From what I have now wrote, *you will see the reason I have to prefer the method of reducing the plants into certain orders (Ordines) rather than to a general system:* because I believe that by this method we will not so easily fall into mistakes of dividing or confounding what should be joined or separated, as we are tempted to do for the sake of a favorite system.

Dr. Colden to Gronovius.

Province of New York, Coldengham, May 30th, 1746.

Sir—The enclosed sheets are a copy of what I sent to you in the beginning of last winter, directed to the care of Mr. Collinson: but as we have heard that the ship was taken betwixt Portsmouth and the Dains, and carried into Dieppe, these papers must be lost. As such misfortune was to be expected, there was an outside direction on the packet in French, desiring the captors in such case to send them to the gentlemen of the Royal Garden at Paris; though I then thought, and still think, that such creatures as privateers commonly are, will very little mind any thing of the kind. But I mention this to you, in case the privateer be a man of some taste for learning, you may take some opportunity of inquiring after them. I sent along with the papers the specimens which you desired, and some others, together with the seeds of several plants; the loss of them I cannot at present repair. Pray God these wars may soon cease, for they are very destructive to learning. [He next announces the reception of a letter from Gronovius, dated July 9th, 1745, as well as the missing packet, despatched in 1743, and mentioned in Gronovius's letter of that year. This third letter of Gronovius, unfortunately, is not to be found among the Colden papers. At the close of this short epistle, Dr. Colden alludes as follows to the philosophical speculations which about this time occupied much of his attention.]

I design to order three copies of a small piece to be put up in this parcel, which I intend to submit to the examination of the learned, the printing of which I hope will be finished before this goes. It is on a subject which has puzzled philosophers in all ages; the solution of which I fancy that I have hit upon, and

that it may be of use in the improvement of knowledge in every part of physics. I know not whether your taste be in this [department ?] of learning; but whether or not, I must beg the favor of you to desire some of your mathematicians, those chiefly versant in the Newtonian and Leibnitzian systems to peruse it, (of which no doubt you have some of distinguished character in your university,) and that you will favor me with your own and their opinion of it, as soon as your conveniency permits. I earnestly beg you to do it without compliment, and with the sincerity and freedom of a friend and philosopher, as you see I endeavor to write to you.

This country is now engaged in a most barbarous war with Indians, popish converts, set on by accursed priests to murder innocent people in their beds, or at their daily labor. Good God, what a religion must that be that incites men to such cruelties! And yet, from what we learn from the public news, your country seems not sufficiently apprehensive of being again subjects of such a bloody and cruel tyranny.

The *piece* referred to in the foregoing letter was doubless his "*Explication of the first causes of action in matter, and of the cause of Gravitation;*" which was first privately printed in New York, and afterwards (in 1746) reprinted in London without the author's knowledge. An enlarged edition, with the author's corrections, was published in London in 1751, with the title, "*The principles of action in matter, the Gravitation of bodies, and the motion of the planets explained from those principles.*" An extract from the preface of this treatise, exhibiting an outline of Mr. Colden's views, to which he attached high importance, is given in *Sparks, Works of Franklin*, Vol. 6, p. 96.

Dr. Colden to Gronovius.

New York, Oct. 1st, 1755.

To Dr. John Frederic Gronovius, Senateur de la ville de Leiden.

It is so long since I had the favor of a line from you, that I have frequently lamented the loss I sustain by being deprived of that correspondence with which you once honored me. Soon after my last to you, my time was so entirely taken up in the public affairs while the last war continued with France, that I

could in no shape continue my botanical amusements. My advanced age, now in the sixty-eighth year of my life, made me think it high time to retire from business, and to indulge the remainder of life in more agreeable pursuits, which require less action than those I formerly engaged in, of which I am become incapable. I am now entirely wrapped up in philosophical amusements, of which perhaps you may see some fruit, if what I have done receive the approbation of those gentlemen to whose judgment it is submitted. * * * But you will perceive by what is inclosed, that botany is not entirely out of my thoughts.

I thought that botany is an amusement which may be made agreeable to the ladies, who are often at a loss to fill up their time. Their natural curiosity, and the pleasure they take in the beauty and variety of dress, seems to fit them for it. The chief reason that few or none of them have hitherto applied themselves to this study, I believe, is because all the books of any value are wrote in Latin, and so filled with technical words, that the obtaining the necessary previous knowledge is so tiresome and disagreeable, that they are discouraged at the first setting out, and give it over before they can receive any pleasure in the pursuit.

I have a daughter who has an inclination to reading, and a curiosity for natural philosophy or natural history, and a sufficient capacity for attaining a competent knowledge. I took the pains to explain Linnæus's system, and to put it in an English form for her use, by freeing it from the technical terms, which was easily done by using two or three words in place of one. She is now grown very fond of the study, and has made such a progress in it as I believe would please you, if you saw her performance. Though perhaps she could not have been persuaded to learn the terms at first, she now understands in some degree Linnæus's characters, notwithstanding that she does not understand Latin. She has already a pretty large volume in writing, of the description of plants. She was shown a method of taking the impression of the leaves on paper with printer's ink, by a simple kind of rolling press, which is of use in distinguishing the species. No description in words alone can give so clear an idea, as when assisted with a picture. She has the impression of three hundred plants in the manner you'll see by the samples. That you may have some conception of her per-

formance and her manner of describing, I propose to inclose some samples in her own writing, some of which I think are new genus's. One is the *Panax foliis ternis ternatis*, in the *Flora Virg.* * * Two more I have not found described any where; and in the others you'll find some things particular, which I think are not taken notice of by any author I have seen. If you think, sir, that she can be of any use to you, she will be extremely pleased at being employed by you, either in sending descriptions, or any seeds you shall desire, or dried specimens of any particular plant you shall mention to me. She has time to apply herself to gratify your curiosity more than I ever had; and now when I have time, the infirmities of age disable me.*

Nothing could oblige me more than your having introduced me to some correspondence with Dr. Linnæus, from whom I have had the honor of some letters. With the last I received the first part of the *Plantæ Coldenghamiæ*, &c., which he has published in a manner very much to my advantage; but I have not seen the second part, which by a line at the end of the first is promised.† I am very unfortunate in not being able to continue any correspondence with him, by the want of every method of conveyance between us. I have attempted it unsuccessfully by way of London, I suspect by my friend ——'s neglect. However, sir, pray when you have an opportunity, make my compliments to Dr. Linnæus, and assure him that no man can have a higher esteem of his great merit than I have, or would more willingly, were it in my power, make a grateful return for the favors I have received. Please to let me know what new things he has done for the information of the world. No doubt he still continues to improve our knowledge, but I am entirely ignorant of every thing lately done by him.

I had the pleasure of conversing with Mr. Kalm, in his passing and repassing through this province; though I was at the time

* Further information respecting Miss Jane Colden, the first botanist of her sex in this country, may be found in the correspondence of Collinson, Garden, and Ellis, with Linnæus, (noticed also in this Journal, Vol. XL, pp. 5–6.) In the second volume of the *Edinburgh Essays and Observations, physical and literary*, 1780, she has very correctly described under the name of *Gardenia*, the *Hypericum Virginicum* of Linnæus, (*Elodea*, Adans.) and skillfully indicated the characters which generically distinguish it from *Hypericum*. Miss Colden died unmarried.—A. G.

† The second part of the *Plantæ Coldenghamiæ*, we believe was never published.—A. G.

very much encumbered in business. As he had the advantage of being thoroughly conversant in the European plants, more than any other person who before him had been in America, I was full of hopes that the American part of botany would be brought to great perfection, and have longed to see his performance in print. However, as many plants must have escaped his researches, much must be left to the industry of those who follow him; whose labor must be much facilitated by what no doubt he has done.

It is so long since I received a line from you, that I know not how you are disposed to receive any thing from me, or that you are now desirous of having some seeds which you mentioned formerly, otherwise they would have been sent at this time. I likewise heard that you was deeply engaged in business.

The two letters which Dr. Colden received from Kalm were written from Philadelphia; the first shortly after his arrival in this country; the second just before his return to Sweden. They are written in tolerable English.

Kalm to Dr. Colden.

Philadelphia, September 29th, 1748.

Dear Sir—I have the honor to send to you the letters of Mr. Linnæus, which he did leave to me the last year, when I went from Sweden. It is about three weeks since I first came to this country. When I first was going from my own country, I thought that I should have the good luck to be here in the beginning of the spring; but great storms in the sea, besides the war, did hinder me from the same. Now, because I do not know if I can have the leisure and advantage to see you, sir, I could not forbear to send you the above mentioned Mr. Linnæus's letter. I can't enough express the kind love and great esteem he have for you. I should too think myself very happy if I could have the honor to see you and pay you my respects; but I can't tell any thing yet, if I am to take the road from hence to New England by water or by land. I am sent of the Royal Academy in Stockholm, to make several observations in the Natural History of the most Northern parts of America, in Botany, Zoology, Astronomy, etc. I have the mind to stay

this next winter in Boston, or perhaps more to North; and the next summer, if God keep me safe, I think to see some part of Canada. In the latter end of the same summer I propose to return to England, and so to my own country. If I did know that you have no examplar of Mr. Linnæus' his *Fauna Suecica*, I could spare you one thereof. I have yet by me one examplar of his *Flora Zeylanica;* if I come to you, sir, you shall have the same too. I took some of his books with me to give to my friends and the lovers of Natural History in this country; which, perhaps, not always can have the same. I have self had the advantage to be by Mr. Linnæus about two years time, and was loved of him as [if] I was his own child. If it happens that I am to take the way through New York, I will pay you my respect. Ignoscas quæso, vir nobilissime, si minus bene lingua vestra . . . . utar, notitia enim omni elegantissima et utilissima hujus lingua penitus carebam, priusquam Anglicus salutas scire terras. I am, dear sir, your most humble servant,

PETER KALM.

P. S. I should be very much obliged to you, sir, if you could procure for me some seeds of the kind of *Acer* which they call *Sugar-Maple*, and whereof the Indians in some places make a sort of sugar.

KALM TO DR. COLDEN.

Philadelphia, January 4th, 1759.

DEAR SIR—Upon my last letter, which I had the honor to write to you, sir, I have not to this day received any answer; so that I am very uneasy, fearing you is not well. I had the honor then to acquaint you, that I had deferred my going home to the month of January, because I was not sure to find any ships going for Sweden at my arrival in London. Now as the ship upon which I intend to go from hence will not be ready to sail before in the middle of February next, I have taken the opportunity to write to you with another gentleman. I should be very glad, sir, if I could receive from you the letter to Mr. Linnæus, which you was so kind and promised to send to him: he will be exceedingly glad of that, as there are few persons he sets such a value upon as upon you.

But excuse me, dear sir, that I again am so bold to trouble you with the same that I before wrote to you.

1. Be pleased to give me a good history of your life; there is nothing we want so much as a *Biographia Botanicorum:* the old were very negligent in that. There are many of which we hardly know any thing but the name; nay, if we shall get to the history of their life, we are obliged to pick up here and there a word in the writings of their contemporaries. At our days we take a more trouble in that. I have already got the history of Mr. Bartram's life, and of Clayton's. I hope Dr. Gronovius will give out his *vitæ historiam.*

2. If it was not too much trouble to give me a catalogue of all the Quadrupeds you have any knowledge of to be here in North America, both wild and tame. My catalogue of them is this; of wild, Panther, Wild cat, Loup, ... Martin, Skunk, Mink, Fischer, Possum, Otter, Seal, Wolf, Red fox, Gray fox, Fox with a cross upon the back, Silver-colored fox, Black fox, White fox, Bear, White bear, Raccoon, Ground-hog, Porcupine, Talpa, Vespertilio, Rabbit, Hare; of Squirrel-kind, the Gray, Red, Black, Flying, Ground, White; Beaver, Musk-rat, Deer, Elg [Elk], Wild oxen of two sorts.

Is there more than one sort of Panther?

Is there more than one sort of Wild-cat? The French in Canada made a distinction between *Chat sauvage* and *Loup* . . .

Is there any *Mustela vulgaris* or Weasel, colore albo, vel alio colore?

Is there any Opossum so far to north as where you lives?

Are the Seals seen in Hudson's River?

Is there more than one sort of Wolfs?

Is there any white Bears different from that at Hudson's Bay?

Is it more than one sort of Porcupine?

Is it more than one sort of Bat, or Vespertilio?

How many sorts of Rat-kind?

Is there more than one sort of Deer? The French make distinction between *Cerf* and *Chevreuil.*

Have you heard any thing of the Moose-deer?

Pray, sir, give me a short catalogue of all the fishes you know to be in fresh water in your province. What is your opinion why people lose their teeths so soon in this country?

Will you not wonder to hear that I have found here in America, growing wild, the *Colocassia,* or *Faba Egyptia veterum;* which is that species of *Nymphæa* which you will find in *Linnæi Flora Zeylanica?*

My respect to Mistress Colden, the Misses, and young Master Colden: I have the honor to be, dear sir, your most humble and obedient servant, PETER KALM.

DR. COLDEN TO KALM.

SIR—I have the favor of yours of the 4th of last month; but that which you mention to have wrote preceding never came to my hands; so that, till I received your last, I did not know whether you had left America last fall, as you proposed, or not. This made me lately write to Mr. Franklin, to know the certainty of it. I heartily wish you a happy voyage home, and that at your return you may receive those rewards which your labors richly deserve. In answer to the questions you put to me, I shall inform you, as far as my knowledge allows me to go, and in such manner as I judge best suits the view of your queries.

As to what you desire to know of myself, though the account would come more properly from another, yet I shall briefly tell you the principal turns of my life. My father was a minister of the church of Scotland, and the oldest minister in it, before he died. He was much esteemed for his piety and strict morals, and had a considerable interest with many of the nobility. I was educated at the University of Edinburgh. My father's view in my education was for the church, as by his interest I could have no doubt of preferment in it. But after I had gone through the usual studies at the University, my inclinations were averse to entering into orders in the church, and I applied myself to the study of physic. I learned the rudiments of botany under Dr. Preston, whose name you'll find in *Ray's Methodus*. I went through a course of anatomy with Dr. Ariskine [Erskine?], and of chemistry with Mr. Wilson; both of them distinguished in their professions at London. The salaries of the ministers in the church of Scotland are very small; and the expense of my education had so far exhausted my father's pocket, that I found it was not in my power to make which it is necessary for a young physician to do in Great Britain, on his first appearing in the world. My mother had a sister in Philadelphia, a widow who had acquired some estate and had no children, and this induced me to try my fortune in America. I arrived at Philadelphia in the year 1710. Upon my arrival I became very in-

quisitive into the American plants; but they were then so little known, and I had so little assistance from my books, that I was soon discouraged. In the year 1715, I returned to Great Britain. I had conversation with Dr. Halley, and other men of learning at London in the mathematics, for my taste chiefly inclined me to that study. I went to Scotland, and married my present wife, and the year following returned to Philadelphia. I fixed [myself] there, with a view to practice physic; but in the year 1718, I had the curiosity to visit New York, without the least thought of changing my place of residence. I visited the then Governor of the place, General Hunter, as it is usual for strangers to do, though I had no manner of acquaintance with him. He received me more kindly than I expected, and though I staid but three days in the place, I was invited by him to particular conversations. General Hunter had served in the army from the time of the Revolution in Great Britain, under King William and the Duke of Marlborough, to the year 1709, when he was made Governor of New York. He had not only distinguished himself in the field, but likewise in the court, among the polite and men of learning. In about a fortnight's time after I had returned to Philadelphia, I very unexpectedly received a letter from Gov. Hunter, with an invitation to come to New York with my family, accompanied with the offer of an office of profit; which I accepted, and soon after removed to New York. Gov. Hunter continued in his government only two years after my removal; but I had the good fortune to be in favor with all his successors, one only excepted. In the year 1722, I was appointed one of the King's Council for the province of New York. The business of my office of Surveyor General of lands obliged me to be much in the country, and my intimacy with the governors occasioning a greater expense than suited my circumstances and tastes, accompanied with no small share of envy, I removed my family, about the year 1739, to the country. After which time I indulged my humor in philosophical amusements more than I could do while in town. It was some years after this, that I accidentally met with Dr. Linnæus's *Genera Plantarum*. I was so much taken with the accuracy of his characters, that I resolved to examine them with the plants that grow near my house; and this is the sole occasion of what you have seen

from me in Botany, and which is so inconsiderable that I can have no pretensions to any merit in the science.*

As to your other queries I can give you but little satisfaction. You know a great deal more than I do of the quadrupeds in America. I never heard, nor did I imagine that we had so many species of Foxes in America as you mention. It is very unhappy that our climate is so fitted to the fox constitution. I know of neither Hare nor Rabbit in this country; what we have is a middle species between the two. I have heard of a white Squirrel. Panthers are so rare that we hear of one only in a dozen years. I have seen two species of the *Mustela;* one, *Mustela fulvo-nigricans inferiore parte capitis, gulæ, abdominis, et interiori femorum alba;* 2. *Mustela tota candidissima excepto cauda apice atro.* This last is the only beast of the ravenous kind that I have a value for; because one or two of them delivered my house and barn from rats, when I was like to be devoured by them. * * * It is a most beautiful white and soft fur, so that I do not doubt of its being the true ermine.

I never saw an Opossum, nor heard of any in this province. I never heard of more than one kind of Wolf, and I suppose that you know the Indian Dog is much shaped like a Wolf. I never saw any Porcupine but in the Mohawks country, nor have I ever heard of any in this part of the country. I know only one sort of Rat; none of the Rat-kind I believe are properly natives of America, but have been all originally imported. I have often heard of the Moose-deer. One, I think, since I came to the country was catched near Albany, but I can give you no description of it. I have heard that it is as large as an ox, and has a mane like a horse. Any country boy you meet with can inform you more of fishes than I can.

As to the reason of the children of the people from Europe (not the native Indians) losing their teeth so commonly, I attribute it entirely to the scurvy, of which scarce one family is free. * * * I have heard that the Indians eat the roots of one kind of *Nymphæa;* but I did not suspect it to be the *Colocassia*, because Linnæus ranks that with the *Arum.* Please to distinguish the species, and tell me the reason you think it the *Colocassia*

* The reader will find another brief autobiography of Dr. Colden, in his letter to Peter Collinson, dated May, 1742.—A. G.

Ægyptiacum. It will give me the greatest pleasure to hear of your safe arrival at home, and that you have published the fruits of your labors in America. Mr. Franklin, at Philadelphia, will take care of any letters for me, or Mr. Collinson in London.

The Colden papers comprise three letters from Linnæus. The earliest is dated at Upsal, on the 6th of August, 1747, and was sent by a clergyman by the name of Sandin, who came to Pennsylvania. It contains a few remarks upon the manuscript *Plantæ Coldenhamiæ*, then in his possession, and a request that he would send dried plants and seeds. The second, without particular date, was written in the same year, and brought by Kalm, and contains many notes and queries respecting the plants of Colden's manuscript. To these, Dr. Colden replied at length in his letter of February 9th, 1748–9 (O. S.), and in another entrusted to Kalm a year after; which having both been published in full by Smith, in his *Correspondence of Linnæus*, need not be reproduced here. The first is chiefly occupied with Colden's views respecting the nature of genera, &c., which are substantially the same with those given in his letters to Gronovius. Linnæus briefly alludes to this subject in the following epistle.

Viro Illustri Convallad. Colden s. pl. d. Car. Linnæus.

Literas tuas vir illustris, 1748–9, Febr. 9 datas, accepi, et summa animi voluptate perlegi, utpote datas a Fautore longe remoto et curiosissimo. Sententiam quam fores de generatione plantarum ad instructionem generum, eadem est quam proposuit D. Mitchel in Actis Naturæ Curiosorum; statuis plantas ejusdem generis esse, quæ possunt genitura sese miscere; at ego has varietates dico, nec distincta genera. Sint exempli gratia Ranunculi species diversæ, quas nullus negabit genere convenire, attamen hæ nulla ratione possunt sese miscere aut una alteram fæcundare; sed Tulipæ [quædam] et Brassicæ, quæ tantum sunt varietates, miscentur facillime.

Dubia et obscura in re herbaria circa terminos et leges varias systematis explicavi in *Philosophia Botanica*, quæ etiamnum sudat, quam cum etiamnum e prelo non prodiit, doleo me hac vice ad te, vir illustris, mittere non posse. Habebis in eo libello omnia dubia enodata, quam primum prodeat.

Mitto *Acta Upsaliensia* pro anno 1743, ut videas primam partem *Descriptionum* Tuarum: altera pars imprimitur in anno 1744, quæ nondum a prelo exiit. Si habes plura mittas quæso, omnia candide actis inseram; utinam velles tum aliquot plantas siccas simul mittere et semina: occasio quotannis datur per theologos nostrates. Si quidquam sit quod in nostris terris desideras, parata tibi sunt omnia quæ a me expetas.

Multæ sunt inter tuas plantas rarissimæ, antea non descriptæ, nobis nec vivæ, nec siccæ visæ; utinam liceret has possidere in herbario nostro. Tu valeas et diu vivas. Has exorare debui ut testarem officia et observantiam meam Mecænatem in Floræ.

Dabam Upsaliæ, 1750, d. 10 Augusti.

Dr. Colden's correspondence with Peter Collinson commenced in the year 1740, and was continued without interruption during the life of that amiable and excellent man. Collinson's last letter is dated July 2d, 1768: he died on the 11th of August following, in the 75th year of his age. The selections I have ventured to make from this voluminous correspondence, form an appropriate supplement to Smith's very interesting collection of the letters of Collinson to Linnæus.

Mr. Collinson to Dr. Colden.

London, March 7th, 1741.

Dear Friend—You have much obliged me by yours of the 22d June, and I am glad to find my little offices were acceptable to you.

I communicated your letter and project* to Mr. Grayham, whose answer I enclose; he has also been so good as to get Mr. Sisson's proposal to make an instrument that will be suitable for your purpose.

I also lent Mr. Grayham your *History of the five Indian Nations:* he was mightily pleased with it, and hoped you would oblige the world with the second part; for that he had not read any that had gave him that satisfaction and information that yours did, because he was persuaded he could depend on your veracity. You really delight me in hopes of seeing the second part; but pray take your time and do it at your leisure.

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* Relative to an improvement in the quadrant, which Dr. Colden had suggested.—A. G.

Pray have you thought, or can you give a conjecture how America was peopled, or was it a separate creation? Most of your vegetables and many of your animals are different from ours, and yet you have some exactly like ours, of which I have specimens by me; for I have a large collection, considering my years and station, of natural varieties, and some artificial, from most parts of the world, which I am obliged to my distant curious friends for sending me. They afford me great entertainment at my leisure hours; and in the country, if I may boast, my garden can show more of your vegetables than perhaps any in this island, which I have been collecting some years from seeds, and growing plants sent me by my friends in your world; so that I am no stranger to America, being pretty well acquainted with most of its productions, whether animal, vegetable, mineral or fossil, perhaps beyond what you can imagine. The uses I make of them is to admire them for the sake of the great and all-wise Creator of them, to enlarge my ideas of his almighty power and goodness to mankind, in making so many things for his profit and his pleasure. I reason on their natures and properties, so far as I am or can be informed; I compare them with ours; in short, I esteem the regard I pay them as a piece of adoration due to their great Author.

Thus, my dear friend, you see I open all my mind to you, and tell you how I employ all my leisure hours, I may say minutes, from business. I hate to be idle, and think all time sadly lost that is not usefully employed; for which reason, clubs, taverns, and coffee-houses, scarcely know me. Home is the most delightful place to me, where I divide my hours in business, in innocent amusements, and in the dear society of a tender, kind, good woman, a boy and a girl. I may now say with Milton, I have now brought you to the state of earthly bliss, and sincerely wish all mankind as happy.

I had a letter from J. Bartram;* he much laments the disappointment of not seeing you. I am persuaded you would have

* In a former letter, Mr. Collinson thus introduces the earliest native American botanist to Mr. Colden's notice.

"If an ingenious man, and a great searcher into nature, named John Bartram of Pennsylvania, should wait on you, please to give him what information you can in those things. He has been a considerable traveller in the world, and is employed by a set of noblemen and others to collect seeds and curiosities for

been pleased with him; you would have found a wonderful natural genius, considering his education, and that he was never out of America, but is an husbandman, and lives on a little estate of his own about five or six miles from Philadelphia, on the river Schuylkill. He really surprised me with a beautiful draught on a sheet of paper of the falls of Mohawk River, which he took when he was there, with a particular account of it, and also a map of his own making of Hudson River, Delaware, and Schuylkill, and the bay, which takes in the provinces of New York, Jerseys, Pennsylvania, Maryland, and part of Virginia; for he has travelled all over these countries yet uninhabited beyond the mountains, as well as the inhabited parts along the bay and the sea-shore, from the Capes to your province. His observations and accounts of all natural productions that happen in his way, (and I believe few escape him,) are much esteemed here for their truth; and he wants not terms to express himself with some accuracy. I have procured him assistance from some curious persons here, to enable him to make further discoveries. Now, my dear friend, I rely on your candor to receive this rambling epistle, as it is intended, in friendly part. From a man much engaged in business correctness is not to be expected; for really I am obliged to write a paragraph now and then, subject to many interruptions.

My best wishes attend you: when leisure offers give a line to your sincere friend, P. Collinson.

Mr. Collinson to Dr. Colden.

London, March 12th, 1742.

My Dear Friend—You have loaded me with many favors. How I shall make ample returns I know not: but if you will allow me time and have patience, I may in some measure testify my gratitude. If I may judge of your disposition, I persuade myself you will prove a merciful creditor, and then by little and little I may discharge my obligations. But at this season is our greatest hurry of business, [so] that I am afraid I shall only be

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them." The next year Mr. Collinson writes—"I have a letter from J. Bartram, who is full of gratitude for the kind reception at Coldengham. Your affable and generous treatment he will never forget; and indeed I have a due sense of the favors shown him, being partly on my recommendation."

able to acknowledge the favor of yours of May 5th, ... with your printing and botanic schemes inclosed.

You will expect I should give you some account of your curious manuscript. I cannot do this to my liking. Our people are so wretched mercenary, that they are unworthy of it. It is now in the hands of an honest, ingenious printer, (if I am not mistaken,) and in my next you will hear more from me. I am persuaded it will meet with approbation from the public; but the way to introduce it is through such wretched, narrow spirited creatures, who are wholly governed by interest, that it is really discouraging for an ingenious man to set pen to paper, if the common good did not counterbalance all other considerations.

I shall at my leisure consider your botanic essays. As to your printing scheme, a printer that is esteemed a knowing man and of great business, gives this answer to it, for it is out of my province. He says your scheme has been tried long ago by good hands, but was found to be expensive and inconvenient in many respects, and at no rate will do for any thing else but bibles, prayer-books, &c., and even in them it is very difficult; for alterations are not easily made, though the author of the scheme thinks otherwise. And as to authors, it would be a discouragement to them; for this method would at least cost them as much as printing a thousand copies in the common way. For the composing is the chief part of the expense, the press-work only coming to a trifle. It would be easy to convince any person of the impracticability of the thing, and the vast expense that would attend it, by a few minutes' conversation.

Now, my dear friend, I confide in your humane and candid disposition to excuse me from adding further, but that I am your obliged and affectionate friend, P. Collinson.

Extract from a draft of a letter from Dr. Colden to Mr. Collinson (without date) in reply to the above.

"It seems that I have the misfortune to trouble you with discoveries which I thought my own, which have been tried by others before. This is owing to my knowing so little of what passes in the world. I must own, however, that a few years since I read in one of the London newspapers that a new method of printing was discovered, which it was thought would be of use in the advancement of learning, and a particular benefit to

authors: but of the manner not the least hint was given. This led me to think what possibly this method might be; on which occasion the scheme which I sent you occurred to my fancy. And as my own appeared to be feasible, and I heard nothing further of that mentioned in the newspaper, I concluded they might be different, and mine of use, though others should have failed. Mr. Graham's arguments fully convinced me that the improvements I proposed for a quadrant cannot answer my expectations; but I cannot say I am as much convinced that the method of printing cannot succeed, from the objection the printer has made to it. For the charge of composing and of the paper being the chief charge in printing, when once composing may serve for an age or longer, and for a hundred editions, it seems to me a strong argument in favor of the new method, rather than an objection to it. But perhaps the prejudice one has in favor of his own conceits, and a jealousy that printers may oppose it with a view to their own interest, may make me a less proper judge in this case. My situation in this country puts it out of my power to make any experiment of this method of printing; otherwise, could I have procured as many types of the fashion and metal which I propose, I should have put the matter out of dispute before I had sent my thoughts of it so far as England. But as the first author of it is in England, and has all the opportunities he can desire to recommend it, it is needless for me to think further of it. Only if you be acquainted with him, you may inform him that if the charge of lead plates be thought too great, that I think the impression from the types may be made on thin boards of lime-tree or poplar; both which are of a smooth grain, very soft while green, and hard when dry: and of any other thoughts I have had on the subject, I shall be well pleased that he make what use of them he thinks proper."

A particular account of Dr. Colden's invention, which resembles the early attempts at stereotype printing, is given in a letter to Dr. Franklin, dated October, 1743, which is published in the *American Medical and Philosophical Register*, (edited by Dr. Hosack and Dr. Francis,) and also in *Sparks, Works of Franklin*, Vol. 6, p. 18. The latter also gives the correspondence with Mr. Strahan (the printer to whom Mr. Collinson submitted the plan) upon this subject.

DR. COLDEN TO MR. COLLINSON.

May, 1742.

DEAR SIR—I never received any thing with more pleasure than yours of the 7th of March last; when I perceived by it that I had gained so great a share of your friendship, and that by such means as I had reason to fear might have deterred you from continuing any further correspondence by the trouble it has given you; and on a subject which proves fruitless, any otherwise than to show how careful a man should be not to be fond of any notions he conceives, or any subject which he does not perfectly understand, and how necessary and useful it is to consult those who are skillful. Mr. Graham in three lines, (masterlike in mechanics,) not only shows that my notions cannot be reduced to practice, which I only before suspected, but that they are likewise faulty in theory in that respect. Pray, sir, return my most humble thanks to him. I can have no hopes of making him any return; otherwise I should with a great deal of cheerfulness offer my service to him in this country. However, I still so far continue my opinion of the difficulties of making small instruments, especially if they be in any manner compounded, and likewise of the difficulty to observe with them to a sufficient accuracy, that I cannot as yet entertain a sufficient esteem of Mr. Sisson's instrument, because of the difficulty of discovering the errors and correcting them, when an entire circle is not used, otherwise than by a long series of observations made with the greatest accuracy.

Last winter I employed the greatest part of my leisure time in revising the first part of my *History of the Five Nations*, and in putting into some order the materials which I had collected about the year 1725 for the continuing of it; and I left so much of it as I could then get copied with Mr. Alexander, in March last, to be sent to you by a ship which proposed to sail for London soon after that time; and since that, I have got the remaining part of it copied, which I now send to him to be forwarded to you. I am truly ashamed that I could not have it copied in a better hand, but in the situation I am in I could not help it. My chief view in that work, I may truly say, is to do you a pleasure. However, if you think it may be useful to the public, you have my full consent to publish it in what manner you think fit; though I have no great fondness to appear as an author, while I am sensible

how much more a man is likely to suffer from the malevolent tempers of many readers, than to gain any applause or benefit from those that are more candid and indulgent, where the design of writing appears to be useful, though it be weakly performed. Every man in my opinion owes so much to his country, that he should patiently submit to scoffs, and jests, and revilings, when he thinks he cannot avoid them by being useful; and I hope it will appear my design is, as it really was, in writing that history, to be in some degree useful to my country. If it be so, I shall truly gain my end, without any further view besides that of endeavoring to give some pleasure and amusement to you.

I once had entertained hopes of enlarging my knowledge of the Indian affairs and manners, by spending some time among them; but as I did not understand their language, and could have no interpreter but at a considerable expense, more than I could bear, I was forced to lay aside that design; and now I have little or no hopes of gaining more information than what I already have. I might have put in several more particulars, to show upon what grounds I have more than once blamed the mismanagement of the Indian affairs in this province; but I did not think it proper to be too particular, as it must throw severe reflections upon particular persons or families now in this province. I may venture to give you in private some particular facts which it is not proper by any means to be made public and general. * * * More such instances can be given. * * *

I look upon it, sir, as one of the happy incidents of my life, that I have had the good fortune to fall into a correspondence with you; because I take you to be one much of my own taste, and I have often wished to communicate some thoughts in natural philosophy, which have remained many years with me undigested; for we scarcely have a man in this country that takes any pleasure in such kind of speculations. Your communicating to me your private manner of life is the strongest instance of your friendship, and in some measure makes up the loss of a personal acquaintance, which I cannot hope to obtain. This encourages me to give you some account of myself, believing you may expect it, as I hope that you intend to continue your correspondence.

I was educated in Scotland, by my parents, with a view to be settled in the church there; and I had as great encourage-

ment in that way by my father's interest, who was a minister of that church, as any young man could have; for my father was acquainted with, and had gained the esteem of many of the nobility and gentry, not only of those who thought as he did in respect to religious principle, but likewise of those who differed widely from him. But my taste and inclinations led my thoughts another way. I applied myself to the study of physic, and, as my father's fortune was not sufficient to enable me to push my fortune in England and Scotland, I went over to Pennsylvania, in the year 1710, where I had some relations. When I came first into America, I was very young,* and though I had some knowledge of books, I was absolutely a stranger to the world. The encouragement to a mere scholar is very small in any part of North America, and I had little sense of the value of money at that time, when it would not have been difficult for me by trade to have raised my fortune, as others did about the same time. I had sufficient for my present occasions, and I had not then learned to be concerned for the future.

In the year 1715 I married, and my family soon began to increase, which gave me some care that I had not before. Soon after this, going out of curiosity to see New York, I fell into Brigadier General Hunter's conversation, who was then governor of that place. He gave me an invitation to settle in New York, with an offer of his friendship, which I accepted. By his interest, I was made surveyor general of the king's lands in this province. Mr. Burnet soon succeeding him as the governor, I likewise gained his friendship, and he recommended me to be of the king's council for this province, in which two offices I have continued ever since. My family being considerably increased, I left the city at the time Mr. Burnet was removed from the government, and settled them in the country, where I now live, as being less expensive. I have been enabled to live above want, to keep free of debt, so as never to suffer a laboring man to go from my house without his wages, and I hope to be able to put my children in a way to provide for themselves by their own industry; which often proves more advantageous to them than leaving such estates as that they can hope to live without thought or care. My eldest son has for some years kept what we call a store in this part of the country. I suppose you know

* He was born, it appears, in the year 1688.—A. G.

of what kind of mercantile business it is, by your general knowledge of America. My eldest daughter is married, as to fortune beyond what I could expect in regard to my own, to one of the late Mr. De Lancy's sons. I doubt not you have heard of his father, he having been one of the most noted merchants in America. My younger children give me reasonable hopes of their doing well in the world, as they grow up, by their industry and virtue.

My removing to the country, I believe, has been of no disadvantage to my children, as it has freed them from many temptations to vice, to which youth is exposed in the city. My chief pleasure, like yours, is in my own family, with my wife and children, and I wish I could live so as never to be from them. I have always had a view to be useful to my country, (though I have had sometimes my designs that way grossly misinterpreted,) and I have taken most pleasure in speculation for that end. I cannot say how far I have succeeded; but none now deny the benefit of the trade at Oswego, in the framing of which scheme and reducing it to practice I had a considerable share. I have made a small spot of the world, which, when I first entered upon it, was the habitation only of wolves and bears and other wild animals, now no unfit habitation for a civilized family; so that I may without vanity take the comfort of not having been entirely useless in my generation. I once intended to have attempted the natural history of this province; and Mr. Burnet for my encouragement annexed a small salary to my office of surveyor general, to be paid out of his majesty's quit-rents. But Mr. H. Walpole at the same time having procured an additional salary as auditor of his majesty's revenue in America, mine was taken off, to make way for his, and I was left without any thing besides the perquisites of my office, which often are very precarious. This obliged me to lay aside all kind of study that was attended with expense of time and money. I hope, notwithstanding of this, to be able to entertain you from time to time with what may prove no disagreeable amusement, according to your own taste. I have at this time too far presumed on your patience; but it now begins to be difficult for me to leave off while I write to you; for I really am, dear sir, your most obliged and affectionately humble servant,

CADWALLADER COLDEN.

MR. COLLINSON TO DR. COLDEN.

London, March 9th, 1743.

DEAR MR. COLDEN—You cannot be more surprised at the progress of botany in Doct. Linnæus, than I am to see what a proficient you are on his scheme. I could not have imagined it had reached in so short a time to the remote parts of North America, for I hear he has made several proselytes in different places on your continent. But your fame reached me long before your letter. My valuable friend, Dr. Gronovius, let me know what a fine present you have made him; the good man is in raptures. I doubt not but Doct. Linnæus has heard of it long before this. I shall soon send your curious observations to Doct. Linnæus; your criticisms are perfectly just; you have done me a pleasure in circulating it through my hands, because it sets me right who have not leisure for such nice observations, and to the Doctor, I know it will give him the greatest delight; any omission in him is not owing to his judgment, but want of growing subjects whose minute parts are more distinct, which are lost in dried specimens; so that at the same time that you are improving your own knowledge, you are greatly obliging your friend; and if all his pupils was equally as communicative as you are, his works would be more perfect and complete. It is no little disadvantage to him to be settled as in the fag end of the world. In his letters to me, he envies our happiness, who have a free and frequent intercourse with your world, and our gardens abound with its productions; and then we have annually seeds and specimens which produce something new, and proper to exercise the talents of so learned and curious a botanist. But a gentleman of your benevolent disposition may in some degree soften the severities of the north, and Flora may, in some little disguise, by your assistance, for once appear amidst ice and snow. A few specimens preserved and dried in paper, and a few seeds, sent him as opportunity offers, with your curious remarks, would be to him all that I have allegorically hinted; and I will take care they shall be safely conveyed to him. * * * I am, with much respect, your affectionate friend,

P. COLLINSON.

Dr. Colden to Mr. Collinson.

Coldengham, Nov. 13th, 1744.

Dear Sir—I have your very kind letter of the 3d of September. If I have had the good fortune to gain your esteem in any degree, and thereby a share in your friendship, I shall think myself well rewarded for any thing I have done; and when I consider the trouble you take, and the concern you have for the little reputation I can hope to obtain, I may flatter myself that I have gained no small share in both. This encourages me to go on in communicating to you what thoughts have occurred to me, which I think can be any way useful in the world or amusing to you. I cannot deprive myself so far of all self-esteem but to hope, that a life of fifty years, a greater part of it spent in some kind of speculation or other, may produce something worthy your inspection at leisure hours, and therefore I shall continue to communicate some thoughts which have at times occurred to me, on every opportunity I shall have of writing to you. * *

The observation you made in your former, that we have in America many different species of plants and animals from those found in Europe or other parts of the world, though under the same climate, is certainly true; and I think we may likewise add, that we have different species of men. This naturally enough leads to the question you put, whether they be the effects of a different creation. But, dear sir, I dare not pretend to give any answer in a matter so high and out of my reach. It is a subject fit to be treated only by first rate philosophers and divines. I should be glad to know your sentiments on it.

I had the pleasure of seeing Mr. Bartram at my house this summer. It is really surprising what knowledge that man has attained merely by the force of industry and his own genius. He has a lively fancy, and a surprising memory and indefatigable disposition. I warned him with some concern against his exposing himself so much to the inclemencies of our climate as he does, and though he thought his constitution proof against it, * * * [As to your] garden I must, dear sir, entreat you to give us some share of that pleasure, by publishing a description of the plants in it, for I cannot hope to partake with you in any other manner. The use you make of it in admiring the infinite variety and beauty of the works of the Creator, comprehends

only one half of our religious duty, contained in Christ's first command to his disciples. It is properly the speculative part of religion which fits us for, and incites us to the practical part, or obedience to his second command, that of loving our neighbor as ourselves. The practice of the second command gives no less pleasure to a good man than the speculations of the first. You have a great deal, sir, in your power; that of being useful to almost one half of the world, to all America. We are very poor in knowledge, and very needy of assistance. Few in America have any taste of botany, and still fewer, if any of these, have ability to form and keep a botanical garden, without which it is impracticable to give complete characters of plants. In short, I may positively assert, that not one in America has both the power and the will for such a performance. Such a work is necessary; it will be a lasting benefit to mankind. It has all the motives to it which can incite a good man to any performance attended with trouble. I am sensible how much your time is taken up with business. But at the same time, I cannot doubt of your obtaining assistance from curious persons, perhaps much at leisure. I told Mr. Bartram of the design I have of entreating you: he was exceedingly pleased with it, and promises all the assistance in his power. * * * How much labor, and how many valuable collections in botany, more than in any other science, have been lost to the world by delay, and an endeavor of the author to complete his work before it appeared in public. I shall not presume to give my thoughts on any particular of the method to be observed in this work, because I have but a very superficial knowledge in botany; I shall only say, that I wish it to be in English, though I know that it is more difficult to do it in this language than in Latin. To encourage you in this, I enclose a description in English of two American plants, not as patterns, but to convince you what may be done, if I, who have so little skill in botany, have been able to make them tolerable. One of them I have for many years taken notice of as one of the signs of a fertile soil; but of late I cannot pass it without paying a particular regard to it. The reason of my choosing the other will appear in the description of it. But to return to the reasons I have for desiring your work in English:—1st. We have nothing in botany tolerably well done in English, so far as I have seen. 2d. It will thereby be more useful in America,

where the learned languages are little understood. 3d. It may set many who do not understand Latin, the ladies especially, on amusing themselves with this study, and thereby procure more assistance in bringing this knowledge to perfection. The ladies are at least as well fitted for this study as the men, by their natural curiosity, and the accuracy and quickness of their sensations. It would give them means of employing many idle hours, both usefully and agreeably. As I cannot doubt that Mrs. Collinson has the same taste of pleasures with you, I am fond to believe that she will with pleasure, save you some trouble in such a work as I propose. No doubt your correspondents inform you of the uses of several plants. I wish something of that may be added; for as most of the plants are new to us, the uses of them must be so likewise. Indeed a plant may be long known, and the use but a late discovery.

This brings to my memory what I have read in Allen's London Dispensatory, under the word *Ipecacuanha*, of a root from Maryland, which in most of the shops had been substituted in place of the true Ipecacuanha, the use of which was forbid by the College of Physicians, on Sir Hans Sloane's information, that it was a kind of *Apocynum*. No doubt the college was in the right to forbid the substituting of one plant in place of another; but I am not well satisfied with the reasons given by Sir Hans, as delivered in that book; viz. that it is a poisonous plant, being a kind of *Apocynum*. Now to this I object, that it is doubtful whether any of the plants which are now known by the name of *Apocynum*, be really the *Apocynum* of Dioscorides, by whose authority alone our *Apocynums* are branded, so far as I know, with the character of poisons. Again: supposing Dioscorides' plant to be truly an *Apocynum*, it does not follow that all the species in America ranked under that genus are in like manner poisonous. Dioscorides says, that his *Apocynum* has a very offensive smell: I know an American species whose flowers smell very agreeably; and may not their virtues likewise differ as much? I think we have strong reasons to judge, that the kind of *Apocynum* substituted in place of Ipecacuanha cannot be poisonous, otherwise it could not so generally have taken its place. Sir Hans likewise affirms, that the roots of a kind of *Apocynum* are commonly vended in New Spain for Ipecacuanha; if so, I doubt the greatest quantity of Ipecacuanha in the shops is from

thence. I have inquired of Mr. Bartram and others, to discover this Maryland Ipecacuanha; but can discover no roots under that name but two, both of them taken notice of by Mr. Clayton in *Gronov. Flora Virgin.* Neither of them can be the plant substituted for Ipecacuanha; because the one hardly works with double the dose of the true Ipecacuanha, and the other (an *Esula*) works violently with half the dose. You will oblige me by describing the species of *Apocynum*, substituted in place of Ipecacuanha, as Sir Hans affirms, or whatever other American plant it be.

I have presumed, I am afraid, too far upon your time and patience; but when I consider that I am grown old before I had the good fortune of any acquaintance with you, and that I can have but few opportunities of continuing it, and that only for a short time, I cannot forbear making the most I can of the opportunities granted me, and beg you will excuse, sir, * * *

MR. COLLINSON TO DR. COLDEN.

London, April 26th, 1745.

MY DEAR FRIEND—I did not expect that I should have found any leisure to answer your kind letter of the 8th of December last; but I will find time to thank you for the perusal of your papers to Dr. Gronovius, which I read over and over, and was much pleased with your reasons and objections; [which] are of such weight as must affect the Linnæan system, and prevent its being universally received. Tournefort and Ray, in my judgment, are much preferable. Take this in general. I wish I could be more particular. I really wonder at your proficiency in so short a time. I transmitted those papers by a safe hand to Dr. Gronovius, who writes many curious remarks on the things sent by J. Bartram, in distinct pages. Could you see them, they would I am sure give you entertainment, and put you in a regular method for future enquiries in some matters. I really don't know such another knowing, indefatigable man, as Doct. Gronovius.

Your brother sent me a letter for you, complaining he had not heard from you, which I enclosed early in the spring to J. Bartram, by Capt. Bream, who I desired to forward it to you.

I am glad to hear of the Philadelphia society. I certainly think it cannot labor long when such wonders are all around

them, ready brought forth to their hands, and to which we are great strangers; but because you see them every day, they are thought common and not worth notice.

Hitherto I have wrote only to blot paper; but now I tell you something new. Doct. Knight, a physician, has found the art of giving such a magnetic power to steel, that the poor old loadstone is put quite out of countenance; his steel magnets act on the needles, and transmit their power to knives, &c., as the loadstone. But he has also shown a secret on the loadstone not known before, by increasing its attractive power to a greater degree, and can at pleasure change the poles how he pleases. Take these examples:—A loadstone of a parallelopiped form he made the opposite end south poles, and the middle quite round all north poles. In another flat stone he made the opposite ends north poles, and the opposite sides south poles. In another loadstone of an irregular flat shape, he made half of each of the flat surfaces a north pole, and the other half a south pole, so as that the two half surfaces opposite each other, should be of a contrary denomination, with many other changes and varieties, showing he had the power to impress the faculty of either pole many parts of the loadstone, with as much ease as a loadstone will influence a needle. I am yours, P. Collinson.

Dr. Gronovius hopes you will continue your remarks, and send him seeds of any of your vegetable productions.

The subjoined postscript to a later letter of Collinson to Dr. Colden, will bring to mind the correspondence of this candid man with Linnæus on the same subject; (vide Smith's *Correspondence* of Linnæus; and this *Journal*, Vol. xl, p. 7.)

"Dr. Linnæus is now publishing his *Species Plantarum*, with many new-coined names, which will so puzzle the science of botany that it will be impracticable to comprehend it. The *Azaleas* he has turned into *Kalmias;* so that every book he prints will require a new edition of his *Nova Genera*, which is a grievous tax and imposition on the public."

Mr. Collinson to Dr. Colden.

London, June 9th, 1755.

I cannot let this ship sail without asking you, how it fares with you this troublesome time? Your situation makes me anxious

for you and your family. Crown Point I may call in your neighborhood: if we are so fortunate to take it, it will be well; but unless we can maintain it and support the country round it, it is probable the French Indians, as well as troops, may come to distress the country round it in revenge.

John Bartram's ingenious son William has sent a pretty map of the Drowned lands, including the mountains and a branch of the Delaware on one side, and North River and the Wallkill on the other; near which, between two rivers, you are pleasantly as well as securely settled, which may preserve you from sudden excursions. As inhabitants increase, the Drowned lands will by degrees be drained and become a most fertile spot.

J. B. has made many curious observations on all the country round, and the course of the rivers, &c. He says the limestone in the vale near the last run in the Wallkill, that is, between the Blue Mountains and Katskill Mountains, is composed of seashells, cockles, clams, &c.: but the most remarkable is below *Gosion*, [Goshen,] where the limestone has the most perfect cockle-shells that ever he saw. If any of these happen in thy way, I should like one or two specimens, as confirmations of the universality of the deluge; and seemingly not a great way from thy house are found the oddest kind of scollop-shells in stone that ever he saw: a sample of these will be acceptable. I have the pleasure to tell you that the *Saracenas* are now in flower, by planting them in moss, in artificial bogs. I had your cranberries fruited last year by the same method. * * * I am, my dear friend, truly yours, P. Collinson.

Mr. Collinson's explanation of the occurrence of marine shells in rocks and strata elevated far above the sea, absurd as it now appears to us, was the prevalent, if not the undisputed hypothesis of that day. Dr. Colden's views, as briefly expressed in the following letter, are far in advance of the age in which he lived. Indeed, I suspect that the records of science, down to a considerably later period, will not be found to furnish an explicit statement on this subject so perfectly in accordance with modern geology.

Dr. Colden to Mr. Collinson.

We have had a very extraordinary summer, the dryest in the latter part of it and hitherto, that ever was known, at least these

forty years past since I came to America. I lost my fruit of all kinds by a frost the last day of May, and many likewise lost their rye, being in blossom at that time. There is something in cold and in frost which we do not understand. I suspect strongly that it rises from the ground, by [its] effects being so very different in different places at a small distance from each other, according to the difference of the soil and situation. The lowest situation and meadow grounds are generally most subject to frost. The difference of a few feet in height is often very sensible in the same farm. The most frequent frosts are in dry summers.

I have not met with any shells since I received your last; though I have frequently seen them near my house. Mr. Bartram some years since carried a considerable piece from my house: as soon as I can get any I shall send some to you. *These shells, and many other marine things found far within land and on the top of mountains, I think prove that those parts where these shells, &c. are found were once under water; but it does not prove that the face of the earth was at that time the same as it is now. I think the contrary, that it must be different now from what it was then, and that this difference probably has happened by great general earthquakes.*

In the bundle with the papers, I have mentioned there are some descriptions of plants by my daughter Jane, which I designed to have sent by a ship from hence to Holland, which sailed a few weeks since; but was prevented in sending them by the sickness of my youngest daughter. I am pleased that they go to you, that you may perceive how far she deserves encouragement, in giving such an example to others of her sex. Please to convey them with my letter to Dr. Gronovius, when you have a proper opportunity.

Mr. Collinson to Dr. Colden.

Ridge Way House, Feb. 25th, 1764.

I am here retired to my sweet and calm old mansion, and from its high elevation look forty or fifty miles round me, on the busy vain world below; envying no man, but truly thankful for the undeserved blessings good Providence hath pleased to confer on me. With a pious mind filled with admiration, I contemplate the glorious constellations above, and the wonders in the vegetable tribes below. I have an assemblage of rare plants from all

quarters, the industrious collection of forty years. Some or other of them, all the year round and all the seasons through, are delighting my eyes; for in the depth of our winter, the plants from the Alps, Siberia, and the mountains of Asia, exhibit their pretty flowers and anticipate the spring; the Black Hellebore, with its large white flowers, the Aconite with its golden clusters—these show themselves before Christmas; for that reason the first is called the Christmas Rose. Primroses and Polyanthus, Wall-flowers, and some Violets, and single Anemones, flower all winter, unless a snow happens to fall, which is seldom. It seems a paradox, (considering our latitude,) to tell foreigners that vegetation never ceases in England. I am this instant come in from seeing your Skunk-weed; its early appearance and its singularly spotted flowers, attract the notice of every one. It hath been now a month in flower; by this you may guess the difference of seasons with you and us. But this winter (if it may be called so) is very different from all that has been remembered. We have had as mild and warm, but then, it hath been dry, sunny, and pleasant; whereas this hath been continued (I may say daily) rains: if a few frosty mornings, certainly rain at night, moist and warm, but attended with hurricane winds, and the air so beclouded it was rare to see the face of the sun. The consequences of such inclement weather hath been more shipwrecks and inundations than ever was known in one winter. It is very affecting to read the very deplorable accounts from time to time: the loss of sheep and cattle drowned, advances much the price of provisions; but, thank God, from the plenty of last year (though a bad harvest) our bread keeps under twelve pence a peck loaf, and we have been able to supply vast quantities to our indigent neighbors. I am assured some weeks eight or ten thousand quarters of wheat have been shipped off for France, Portugal, Spain, and Italy: this trade brings in great riches, being a surplus that we can spare without prejudice to ourselves. When I look back and consider the poor state of agriculture here in the last century, it affords a pleasure I cannot express, to see our extensive improvements made in this age. Then the city of London imported annually Polish wheat from Dantzic, (to the enriching foreigners,) to fill our granaries; for our culture of wheat was so sparing, that if a crop failed a famine was like to ensue. To prevent this terrible calamity, the city prudently sold it out one

year under another. But the Dantzic corn trade hath been long left off, and instead of buying, we annually sell to our neighbors.

As often as I survey my garden and plantations, it reminds me of my absent friends by their living donations. See there, my honorable friend, Governor Colden: how thrifty they look. Sir, I see nothing but two fine trees, a Spruce and a Larch. That is true, but they are his representatives. But see, close by, how my Lord Northumberland aspires in that curious Fir from Mount Ida: look yonder at the late benevolent Duke of Richmond; his everlasting Cedars of Lebanon will endure when you and I and he are forgot: see with what vigor they tower away, how their stems enlarge, and their branches extend. But pray what are those Pines? Novelties rarely seen; that elegant one with five leaves is the Cembro Pine from Siberia: the other tall tree is the very long-leaved Pine of ten or twelve inches from South Carolina; they stand mementos of my generous friend, the late Duke of Argyle. That gentle tree, so like a Cypress, looks uncommon: that is the Lycian Cedar; the seed was given me by Sir Charles Wager, first Lord of the Admiralty, gathered in the Isle of Ivica, in his voyage to convey Don Carlos (the now king of Spain) to Naples. But those Balm Gilead firs grow at a surprising rate, it is pleasant to see: they renew a concern for my dear friend, Lord Petre; they came young from his nurseries, with all the species of Virginia Pines and Cedars. That Fir that grows near them is remarkable for its bluish-green; that was a present from my worthy friend, Sir Harry Trelawny: it is called the Black Spruce, he had it from Newfoundland; it grows delightfully. Regard the variety of trees and shrubs in this plantation, as Mountain Magnolia, Sarsifax, Rhododendrons, Kalmias, and Azaleas, &c. &c.: all are the bounty of my curious botanic friend, J. Bartram of Philadelphia. And for these pretty Fringe trees, Halesias, and Stuartia, all great beauties, I must thank my friend John Clayton, the great botanist of America. How fragrant that Allspice; how charming the Red-flowering Acacia, the great Laurel-leafed Magnolia, Umbrella Magnolia, and Loblolly Bay: these charming trees are the glory of my garden, and the trophies of that friendship that subsists between me and my very obliged friend, J. Lambol, Esq. of South Carolina.

Thus gratitude prompts me to celebrate the memory of my friends, amongst whom you have long claimed the respect and esteem of yours sincerely, P. Collinson.

MR. COLLINSON TO DR. COLDEN.

London, Feb. 10th, 1768.

I had the pleasure of my dear friend's letter, with the packet for Lord Shelbourne, which was delivered. Now there is Lord Hillsborough appointed secretary for the colonies; for the future your application will be to him. We may now hope, as the colonies are his peculiar care, the public grievances will be redressed, yours in particular; so the sooner your application to him the better, and if it was conveyed by your friend, Sir Jeffrey Amherst, it may have more weight; for I have no acquaintance with him. * * *

I presume you have heard of the wonderful discoveries. Near the Ohio, about six hundred miles below Pittsburgh, and four from the river, is a great Licking-place: [here] George Crogan, Esq. found a great quantity of the great long elephant's teeth and bones. He sent over of these great long teeth or tusks six or eight, which I have seen and handled. Some about six feet long, and the same thickness as recent elephant's teeth of that size, and, what is remarkable, some are not in the least decayed: the ends cut off show as fine white ivory as recent ivory. Now is not this wonderful! A small tooth about two feet long, of a chestnut color, hath a fine natural polish, as if just taken out of the head of the young animal.

As there never were any elephants in America, neither could they subsist for the severe long winters where they are now found near the Ohio, what hypothesis can be formed to account for these being found there, under a bank on the sides of this great lick, where some portions of the bones and teeth lay exposed to view? (seven hundred miles from the sea.) Mr. Crogan believes, from the quantity of the bones and teeth, there could not be less than thirty animals. He sent the teeth to Lord Shelbourne and Benjamin Franklin. Portions of the like elephant's teeth found in Peru, were sent last year as a present to the Royal Society. Mr. Franklin talks of coming over by some of the summer ships: he is very well, and much caressed and admired here.

Pray give my kind respects to thy son David. I shall be greatly obliged to him for the information he intends me on the Rattlesnake. Any remarks on natural history will be very acceptable to your sincere friend, P. COLLINSON.

P. S. I thank good Providence I have lived to see a pair of your great Moose-Deer's horns sent to the Duke of Richmond: there is not a pair in the British Museum, which is a great loss to that grand collection, which is the wonder of the world.

It was always said the great Deer's horns found in the bogs in Ireland, some ten feet from tip to tip, was the same as the great Moose-Deer's of New England and Canada. But this pair shows there is no affinity; but your Moose horns are very like the Elk of Germany and Russia; so that the animal that produced the Irish horns is not now known to exist in all your discovered world, and it is not in our own parts; but possibly it may have being in Terra Australis, *or no where:* but that is not agreeable to the plan of Providence.

We have no room left for the letters of Dr. Garden of South Carolina; which, however, are for the most part so much occupied with private and colonial affairs, that they do not possess the same scientific interest as his published correspondence with Ellis and Linnæus. The following letter gives an account of his visit to the elder Bartram, at his well-known garden near Philadelphia.

DR. GARDEN TO DR. COLDEN.

Philadelphia, Nov. 4, 1754.

HONORED SIR—I cannot help, once before leaving Philadelphia, begging permission to intrude on a philosophic hour, in troubling you with the perusal of a few lines, according to my promise when I had the pleasure of seeing you at New York. Since my leaving that place I have met with very little new in the botanic way, unless your acquaintance Bartram, who is what he is, and whose acquaintance alone makes amends for other disappointments in that way. I first waited on him with Gov. Tinker and Dr. Bond, whom he received with so much ease, gaiety and happy alacrity, and invited to dine with so much rural vivacity, that every one were agreeably pleased and surprised. Unluckily Gov. Tinker had engaged some company to be with him that day, else we should have taken part of his botanic treat, which he seems fully designed to have some day this week.

One day he dragged me out of town, and entertained me so agreeably with some elevated botanical thoughts, on oaks, ferns,

rocks, &c., that I forgot I was hungry till we landed in his house about four miles from town. There was no parting with him for two days, during which time I breakfasted, dined and supped, slept, and was regaled on botany and mineralogy, in which he has some excellent notions and grand thoughts. His garden is a perfect portraiture of himself; here you meet with a row of rare plants almost covered over with weeds, here with a beautiful shrub, even luxuriant amongst briars, and in another corner an elegant and lofty tree lost in common thicket. On our way from town to his house, he carried me to several rocks and dens, where he showed me some of his rare plants, which he had brought from the mountains, &c. In a word, he disdains to have a garden less than Pennsylvania, and every den is an arbor, every run of water a canal, and every small level spot a parterre, where he nurses up some of his idol flowers, and cultivates his darling productions. He had many plants whose names he did not know, most or all of which I had seen and knew them; on the other hand, he had several I had not seen, and some I never heard of. To-night I shall pay him a visit along with a Jamaica doctor; we set away after dinner, and design to remain all night with him.

I shall be glad to hear of Miss Colden's improvements, which no doubt increase every day, and may we again be surprised with more than a *Dacier*, even in America. Messrs. Wragg and Cleland left this place last Monday; we shall leave it in ten days. I shall expect the favor of a line from you soon, by the time I get to Charleston. I beg leave to offer my compliments to Mrs. Colden and your good family, and remain with great esteem, honored sir, your most obliged and very humble servant,

A. Alexander Garden.

Dr. Colden was appointed lieutenant governor of New York in 1761; and performed the duties of governor for a great part of the time, until 1775, when by the return of Governor Tryon, he was relieved from official cares. He then retired to a seat on Long Island, where he died on the 28th of September, in the 89th year of his age. "For the great variety and extent of his learning, his unwearied research, his talents, and the public sphere which he filled, Cadwallader Colden may be justly placed in a high rank among the distinguished men of his time."

ART. XIII.—*Notice of newly discovered Fish Beds and a Fossil Foot Mark in the Red Sandstone Formation of New Jersey;* by W. C. REDFIELD.

SINCE the discoveries of fossil fishes and foot marks in the red sandstone formation of Connecticut and Massachusetts, some of which fossils resemble in their characters the ichthyolites which are peculiar to the new red formations of England and Germany, it has been deemed important to ascertain if analogous memorials belong to the apparently kindred rocks in New Jersey, and which also extend southwesterly across several of the Atlantic states.

About four years since, the discovery of fossil fishes in the New Jersey rocks, near Boonton, was made known in this Journal. Soon after, I obtained several species from this bed, nearly all of which have been found to agree with the species which I had obtained from the sandstone formation in Connecticut.*

This agreement in fossils being established, it was still desirable to obtain further comparisons, and more especially, to determine whether the fossil footsteps, the *Ornithoid-ichnites* of Prof. Hitchcock, appear in the sandstone rocks of New Jersey.†

Early in the present autumn, specimens of ichthyolites from another locality in New Jersey, were presented to the New York Lyceum of Natural History, by A. R. Thomson, Esq. The specimens were obtained near the sandstone quarries of Peter M. Ryerson, Esq., in Pompton, about twenty five miles from New York, and perhaps ten miles northeasterly from the above mentioned fish-bed at Boonton. These fossils had been lately brought to light by excavations made in search of coal, in a thin bed of dark colored shale, the joints of fracture in which were found to contain indurated bitumen; which had doubtless exuded from the rock, in a softer state. This bed of shale separates the overlying "variegated calcareous conglomerate" of Professor Rogers, from the red sandstone rocks beneath; and is described by him

* This Journal, Vol. xxxv, for 1839, p. 192:—Vol. xxxvi, p. 186:—Vol. xli, pp. 25-28.

† The term Ornithichnites, used by Professor Hitchcock in describing these foot marks, has been changed by him to Ornithoidichnites. See his late Report on the Geology of Massachusetts.

as "a thin bed of gray siliceous slate, very schistose," and, with the other rocks, dipping conformably "to the northwest."*

I have since visited this locality and obtained seven species of ichthyolites, all of the genus Palæoniscus. Three of these, viz. *P. fultus*, *P. latus*, and *P. Agassizii*, are common also to all the fish-beds of this formation in New England. Some of the remaining species seem nearly allied to *Palæonisci* of the magnesian limestone, in the New Red group of England; as appears from specimens sent me by Sir Philip Egerton, as well as from those which are figured in the *Poissons Fossiles* of Agassiz. The genus Catopterus, which is common at Boonton as well as in Connecticut, has not been found at Pompton. Of the several species which belong to the Connecticut localities no less than eight are found at Boonton; which comprise nearly the whole series, in both cases.

The ravine in which Mr. Ryerson's quarries are situated exhibits a good section of this part of the formation; the rocks being here exposed for several hundred feet in thickness, in the order of superposition. In passing up this ravine, we arrive at another thin bed of bituminous shale which dips perhaps two hundred feet below the fish stratum above described. A short examination of this inferior bed sufficed to show that it also contains ichthyolites; a specimen having been fortunately obtained, as I was on the point of leaving the ground. This is the first instance, perhaps, in which a plurality of fish beds, in the order of stratification, has been distinctly observed in the red sandstone formation of this country.

a a Red Sandstone. *b b* Fish Beds. *c* Calcareous Conglomerate. *d* Drift. *e e* Quarries.

While examining the sandstone quarries, which lie between the two beds of shale noticed above, I was fortunate enough to discover and obtain a well characterized foot print. This ichnolite belongs to a species which is found in the sandstone rocks about Middletown, in Connecticut. The specimen is in relief;

* See the final Report of Professor Rogers on the Geology of New Jersey, p. 125, *et seq.*

its character, tripartite, massive, or thick-toed, the two lateral toes very short compared with the middle one; length about six inches, breadth three and a half inches. The claws or nails of the lateral toes are well defined, and project from the bottom of the toe about four tenths of an inch. It appears to be a common variety of the *O. tuberosus* of Prof. Hitchcock, and is nearly represented in figure 21, Plate 37, of his final Report, in quarto, on the Geology of Massachusetts.

In the same quarries I observed numerous slabs and fragments which exhibit fossil impressions of rain drops, or hail; and also natural casts of the same in relief. There is great variety in these appearances; and some are of the most perfect character: while, in not a few cases, the oblique form of the impressions serves to show that the drops were driven by a strong wind. These impressions and their casts seem to indicate a nearly horizontal surface as pertaining to these beds, at the period when the impressions were made and overlaid by the superior deposits. The foot mark, also, exhibits no lateral inclination or obliquity in its form, that might correspond with the present high slope of these strata, which I estimate at about twenty degrees.

I obtained also a good specimen of ripple mark at these quarries. This appears to be not wind-ripple but water-ripple; and on the lower surface of this slab, within half an inch of the ripple, some marks of shrinkage cracks may be seen, such as occur in a drying surface of mud or silt. From this we may infer that the current of water which caused the ripple was produced by rain, or other transient cause.

The facts above noticed seem to render it probable that many foot marks, of various species, will be found in the red sandstone rocks of New Jersey; and that the time is fast approaching when the relations of this formation to those of the New Red system in European countries, can be profitably examined. Indeed, the considerations already adduced by Professor Hitchcock go far to establish the identity or equivalence of these formations.

New York, November 10th, 1842.

ART. XIV.—*Strictures on Prof. Dove's Essay "On the Law of Storms;"* by ROBERT HARE, M. D., Professor of Chemistry in the University of Pennsylvania.*

TO THE EDITORS OF THE AMERICAN JOURNAL OF SCIENCE.

Dear Sirs—Since forwarding my communication containing additional objections to Mr. Redfield's theory, I have perused Prof. Dove's Essay on the Law of Storms. I now send to you for the Journal, some strictures to which I conceive that essay liable.

But first allow me to take due notice of the note subjoined by you to the first page of my last communication, in which you allege your understanding of Mr. Redfield to be, that he represents "*the whirlwind as the cause of the violence, not the cause of the whirlwind.*" The language on which my understanding was founded, represents "a rotative movement of *unmeasured violence*," as "the *only cause of violent and destructive winds or tempests.*" But admitting your impression to be correct, does it make the error less to say that a whirlwind is the only cause of its own violence? Besides, where is the difference between producing a whirlwind and producing the *violence* of a whirlwind? Is it contended by Mr. Redfield that there are two causes, one producing wind, the other the force or violence of wind? But is there not a great mistake made by Mr. Redfield and other advocates of the whirlwind theory, in treating gyratory motion as a *cause* of *violence?* - Is it not evident, that whatever may be the cause or causes of aerial currents, gyration, instead of accelerating that velocity on which violence is dependent, must, by the expenditure of momentum resulting from collision with inert portions of the atmosphere consequent to centrifugal force, cause a great loss of velocity. (See my objections, par. 65.)

100. I have not been enabled to discover that Prof. Dove attempts to assign any cause for violent winds. Assuming that a wind, sufficiently violent, is blowing from south to north, he ingeniously makes a new application of the old doctrine of Halley, by which the westerly motion of the trade winds is ascribed to

* We had hoped to have published Prof. Dove's Essay, on which Dr. Hare comments, in connection with these 'strictures,' but are obliged from the crowded state of the present number to postpone it to our next.—EDS.

the diversity of the velocity of the earth's surface, at different distances from the equator, operating upon a wind blowing from one parallel of latitude to another. I am however unable to understand how any difference of momentum, thus arising, can act throughout all parts of a circle upon an elastic fluid, so as to sustain the equability of motion requisite to enduring gyration. It seems to me that the influence of the terrestrial motion can operate harmoniously neither upon each quadrant, nor each zone of a circle. The effect upon the south limb cannot, I think, coöperate with that upon the northern one.

101. Moreover, as the velocities of the aeriform particles in a whirlwind must be greater as they are farther from their axis, I do not see how a uniform force operating upon particles requiring such various velocities, can produce movements which can harmonize in causing a non-conflicting rotation of the whole mass.

102. How can this process avail to produce a revolution in the same direction in all the storms of this quarter of the globe, as alleged by Mr. Redfield and sanctioned by the author, when, agreeably to the most ample and satisfactory evidence adduced by Prof. Loomis, as well as general experience, some of the most violent storms of this continent travel from the northwest towards the southeast. In such cases, on account of its blowing obliquely towards or from the equator, the wind would change its position relatively thereto, only with a portion of the speed which is assumed in the calculation of Prof. Dove, and when the change of position should be in an opposite direction from that which he supposes, would it not cause the storm to whirl in the opposite way?

103. Prof. Dove in the second paragraph, page 211, employs the following language: "*As the West India hurricanes originate at the inner boundary of the trade winds, where, at the so called region of calms, the air ascends and flows over the trade in an opposite direction; it is probable that portions of this upper current, penetrating through the lower one, can give the first occasion of those storms. The high mountains of several of the islands, by offering a mechanical impediment, may be one cause of this effect, as the air flows with redoubled violence between two mountains.*"

104. Prof. Dove here alleges that the upper current *may* penetrate the lower, but does not say why it should do so. Where-

fore, it may be demanded, should the upper current penetrate through the lower current, and supposing it to do so, why should it be productive of a hurricane?

105. The professor goes on to say—"*It is evident, that if the above deduction of these phenomena be the true one, a similar whirlwind must be produced wherever, owing to any other mechanical cause, a current flowing towards a high northern latitude is more southerly on its eastern side than on its western.*"

106. It seems as if Prof. Dove, no less than Mr. Redfield, falls into the error of making the *cause of gyration* the only object of inquiry. It is, according to them, sufficient to show that the rotation of the earth, or the reaction of a mountain, may give a curvilinear direction to the wind. To account for the wind itself is not in the least necessary!

107. Can any thing be more inconceivable, than that a current of air, not previously moving with the force of a hurricane, should, by influence of the earth's motion, or a conflict with one or more mountains, be excited into a tempestuous fury? Whence comes the alleged peculiar violence of the whirling portion of the atmosphere noticed in such storms? Evidently *deflection* could not cause any *augmentation* of force. The velocity of the whirl would be less instead of greater than that of the generating gale, since the centrifugal force consequent to rotary motion would be productive of a collision with the surrounding atmosphere, tending to dissipate the momentum. This, as I have already observed, could receive no reinforcement, while the mass actuated by it would increase with the square of the distance from the axis. (See additional objections to Redfield's theory, par. 65.)

108. Prof. Dove has not considered the incompetency of a *local* cause of deflection, to beget permanency of rotation in a *travelling* storm; nor the impossibility of the endurance of a momentum sufficient to cause the violence of hurricanes without continuous exciting forces.

109. In a passage which I shall in the next place quote, the idea is advanced that the axis of a whirlwind may incline forward so as to cause the higher portion to precede the lower, and to make the lower stratum of the air forming the whirl exchange places with the upper stratum. This view of the phenomena I shall endeavor to prove erroneous.

110. Page 215, paragraph 3d, Prof. Dove has thus expressed himself: "*In considering the progressive advance of the whirlwind, we have not hitherto taken into account the resistance opposed to the motion of the air by the surface of the earth. This resistance, as Redfield justly remarks, causes the rotating cylinder to incline forward in the direction of its advance, so that at any station the whirlwind begins in the higher regions of the atmosphere before it is felt at the surface of the earth, where therefore the sinking of the barometer indicates its near approach. The inclined position of the axis causes a continual intermixture of the lower and warmer strata of the air with the upper and colder ones, thereby occasioning heavy falls of rain and proportionably violent electric explosions.*"

111. In order to appreciate the fallacy of the ideas above presented, it should be recollected that the "*rotating cylinder*" of air, which is represented as *inclining forward*, can receive this name only because the portion of the atmosphere of which it is imagined to be formed, is conceived to revolve within that cylindrical space which must of necessity be occupied by a whirlwind. To justify this appellation, the gyrating particles must all move in concentric circles about a common axis, and between places parallel to each other and at right angles to that axis. Any other rotative position of the parts must be inconsistent with enduring rotation, since it would bring different parts of the mass in collision with each other and with the air beyond the sphere of the gyration. It should be recollected, also, that agreeably to observation, hurricanes have been estimated to extend from one hundred to six hundred miles in breadth. Let us assume the diameter of a whirlwind storm of this kind to be three hundred and sixty miles. Of course the circumference being about three times as great would have three miles for every degree. It follows, that a vertical circle of the diameter of the storm, in a plane coinciding with the axis, would also have three miles for every degree. The altitude of storm-winds is well known not to be above two miles, so that the diameter would be at least one hundred and eighty times as great as the altitude of the axis. Can such a cylindrical mass of air be conceived to incline forward? One degree of inclination would lift the base in the rear three miles, and two degrees would lift it to the height of six miles, which is never attained by clouds. Besides, as the

density of the air in regions so elevated is only one half of that upon the earth's surface, is it conceivable that a whirlwind could consist of materials so disproportioned in weight?

112. Can the suggested process of circulation proceed when, in order for the lower stratum to exchange places with the upper, it would have to move nearly half of the circumference of the storm, or more than five hundred miles?

113. It has been shown that the *rotating cylinder* of air which constitutes a hurricane, agreeably to the language employed, and the theory espoused by Prof. Dove, may consistently with the observed dimensions of storms, have a diameter two hundred times as great as its altitude. The base, of this flat cylindrical aeriform mass, must be in contact with the terrestrial surface, and of course in collision with its ruggosities and inequalities, while all the rest of the rotating superficies, being contiguous to inert particles of the atmosphere, must incessantly share with them any received momentum. Is there any known cause of motion in nature, which can impart to a fluid and elastic mass so formed, composed and situated, the various velocities necessary to that simultaneous rotation of the whole which the creation of a whirlwind requires? In answering this question, it should be recollected, that the velocities must diminish from the zones of which the gyration is most rapid, towards the axis on one side, towards the circumference on the other.

114. Evidently no transient impulses can produce harmonious revolution throughout the mass, unless they act upon every particle so as to impart to each the peculiar velocity which its distance from the axis may require; and any enduring cause operating partially, could only affect the whole by a gradual process of participation which would cause it to be expanded beyond as well as within any "*rotating cylinder*" which might be created.

115. But admitting that in such a mass, under such circumstances, the gyratory violence of a hurricane could be induced, could this violence be sustained, after the cessation of the generating forces, merely by the rotatory momentum of an enormous aeriform disk, formed, proportioned, supported, and surrounded as the whirlwind above imagined must be, could any such exist?

116. One of the grounds taken by Prof. Dove, appears to me strikingly untenable. His language, Vol. III, p. 214, last paragraph, is as follows. "*The dead calm, suddenly interrupting*

the fiercest raging of the storm from opposite directions, which is shown in the register of observations at St. Thomas; that dreadful pause which fills the heart of the bravest sailor with awe and fearful expectation, receives a simple explanation on the rotary theory, but appears irreconcilable with the supposition of a centripetal inblowing, because two winds blowing towards each other from opposite directions must gradually neutralize each other, and thus their intensity must diminish more and more in approaching their place of meeting. This takes place on a great scale as respects the trade winds, and if the centripetal view of hurricanes were a just one, the same effect would be necessarily seen as the centre of the storm passed over the station of observation. But the phenomena shewn by observation are widely different. At St. Thomas, the violence of the tempest was constantly increasing up to 7h. 30m. A. M., when a dead calm succeeded, and 8h. 10m. the hurricane recommenced as suddenly as it had intermitted. How can this be reconciled with the meeting of two winds?"

117. I have made the preceding quotation from Prof. Dove's essay, conceiving it to contain evidence which must be fatal to the hypothesis which it is intended to prop. It establishes that in hurricanes the wind is liable *suddenly* to subside from its extreme violence to a calm, and then as suddenly to recommence blowing with as great violence as ever in an opposite direction. I am very much mistaken if I have not in my *additional objections to Redfield's theory* (67) demonstrated that, in *extensive* whirlwinds, the "*fiercest raging*" cannot be suddenly interrupted so as to leave a dead calm during the interval which takes place between two opposite winds; since in such storms, where they have a diameter not less than three hundred miles, for the same station to be exposed successively on opposite sides of the zone, where the wind is most violent, the storm must move at least one hundred miles, which would require from three to four hours.

118. Referring the reader to the essay above mentioned, I will urge, in reply to the query already quoted, that Prof. Dove's allegation that "*winds blowing* from opposite quarters *will neutralize each other*," arises from his forgetting that agreeably to the hypothesis which he is striving to confute, they are caused by a deficiency of pressure at the axis of the storm producing an up-

ward current for the supply of which they are required. It could not but be admitted by the learned professor, that when, under such circumstances, a fluid rushes from all quarters towards a focal area, the consequent motion must quicken as it approaches the ascending current. It must also be clear that when it moves from all parts of the circumference, the velocity must increase inversely as the square of the distance from the centre. Nevertheless, after the base of the ascending column is reached, evidently the horizontal afflux must be superseded by a vertical movement. Hence about the centre of the space around which the upward currents prevail there may be a calm.

119. It seems to be conceded that a tropical hurricane is a gigantic tornado. Of course it may be assumed that the features of these meteors are proportionable; and that the focal area of a hurricane, will be as many times greater than that of a tornado, as the whole area of the former is greater than that of the latter. In fact, if the focal areas be respectively bases of ascending columns moving with equal velocities, the quantity of air requisite to supply the upward currents thus constituted, will be as the squares of the diameters of the columns severally.

120. The diameters of the focal areas of tornadoes as observed in this country, seem in no instance to have exceeded five hundred feet. The focal area of the Providence tornado was estimated to be three hundred feet in diameter.* To supply an upward columnar current of ten thousand feet diameter, would require four hundred times as much wind as to supply an analogous current of a diameter of five hundred feet. It follows that a hurricane, equivalent to four hundred of the largest tornadoes, would not require a focal area greater than two miles in breadth. To cross this at the rate of progression attributed to great storms, by Prof. Dove, (thirty miles per hour,) only four minutes would be necessary.

121. Let us suppose the focal area of a hurricane for the base of a vertical current to be in diameter ten thousand feet, and that the space beyond the area be divided into zones by the circumferences of equidistant circles, concentric with each other and with the focal area. The circumferences being equidistant, the quantity of air over these zones will evidently be as the squares

* See letter from Z. Allen, Esq. in this Journal, Vol. xxxviii, p. 76.

of their mean diameters. Of course if the zone nearest the area, having a mean diameter of ten thousand five hundred feet, move inwards with the velocity of one hundred miles per hour, the velocity at four times that distance, or forty two thousand feet, cannot be more than one sixteenth as great, or little more than six miles per hour. Thus at only four miles from the centre, the centrifugal velocity would scarcely be adequate to a breeze.

122. This calculation, founded on the idea of the confluence of the air equally from all points of the periphery, would seem too much to contract the theatre of great storms; but in point of fact, it probably never happens that there is a confluence of the wind from all quarters. In the storm of which the phenomena are so well recorded by Prof. Loomis, the wind blew principally from two opposite quadrants. But in either case the influence of the inward suction must diverge and diminish in force as the distance from the focal area increases, so that the greatest violence will be in the vicinity of its border, where the wind is most concentrated. For as soon as the confluent currents get within the border, they must be deflected upwards; and thus the central space must escape their influence; excepting the diminution of pressure consequent to the upward motion.

123. It is I hope thus rendered evident, that the facts adduced, in the quotation above made from Prof. Dove's essay, are quite consistent with the idea of winds rushing towards a focal area, while they are utterly irreconcilable with that in support of which they have been brought forward.

124. I would recommend Prof. Loomis's* observations to the candid attention of Prof. Dove, and would request him to show in what manner the earth's motion coöperated to produce it; or how the enormous length of the focal area, or area of minimum pressure, comparatively with its breadth, can be reconciled with the idea of its having formed the centre of an extensive whirlwind. There is another fact which would seem to be literally an unsurmountable obstacle to the rotation of a storm travelling from the valley of the Mississippi to the Atlantic coast. I allude to the interposition of the Alleghany mountains. Prof. Dove's imaginary aerial cylinder would be cut nearly in twain when bestriding

* See American Philosophical Transactions for account of the storm of December, 1836.

that range. Obviously more than one half of the air in such a cylindrical mass would be below the average level of the summits of those mountains. Under such circumstances could it be conceived to rotate about a vertical axis?

125. I am aware that various writers have referred to the little *transient* whirls which are occasionally seen to take place in a windy time, carrying up dust, leaves, and other light bodies, as a support for the idea of whirlwind storms; and Mr. Redfield has alleged, "*that no valid reason can be given why larger masses of air may not acquire and develope similar rotative movements.*"

126. It appears to me that there are several valid reasons for not adopting the view of the subject which he has taken. The momentum by which any body is kept in motion, is as its weight multiplied by its velocity, while the expenditure of momentum is *cæteris paribus* as its surface. On this account, a globe of which the content in proportion to its superficies is preëminently great, will, in a resisting medium like the air, retain a rotary motion longer than an equal weight, of the matter forming it, in any other shape. The flat cylinder, in diameter about two hundred times its thickness, of which the existence would be necessary to an extensive whirlwind, is a form of which the surface would be very great in proportion to the quantity of matter which it contains. No observer ever noticed any whirl produced as above described, to have a diameter many times greater than its height, or to endure many minutes. Such pigmy whirls appear to be the consequence of eddies resulting from the conflict with each other, or with various impediments, of puffs or flaws of wind. No doubt in this way a deficit of local density is easily caused in a fluid so elastic as the air, and consequently by gravity as well as its elastic reaction, a centripetal motion is induced in the surrounding aerial particles. From the confluence and conflict of the air thus put into motion, a whirl may arise. The manner in which light bodies are gathered towards the axis of these whirls, shows that they are accompanied by a centripetal tendency. It is only when the wind blows briskly that such whirls are ever seen to take place, but tornadoes agreeably to universal observation occur when there is little or no wind externally. (See objections, par. 76.)

127. According to the evidence adduced by the advocates of the whirlwind theory, there is in this respect perfect similarity

in the phenomena of tornadoes and hurricanes. Beyond the sphere of the alleged gyration, there is but little if any atmospheric commotion, and certainly none competent to be the cause of a great whirlwind. It follows that pigmy whirlwinds and hurricanes can have no analogy. The former are never produced without a proportionable external activity in the wind, while comparative external quiescence seems to accompany the latter.

128. I will conclude by applying to Prof. Dove the stricture which I applied, on a former occasion, to Espy, and to Redfield. He has, I think, committed a great oversight in neglecting to take into consideration the agency of electricity in the generation of storms.

ART. XV.—*Striæ and Furrows of the Polished Rocks of Western New York;* by Prof. C. DEWEY.

Two varieties of limestone underlie the grounds of Rochester and its vicinity. The principal one is the calciferous slate of Eaton, the limestone associated with the *Rochester shales* in the geological reports of the State. The other lies upon the preceding over a small extent, forming a stratum a part of a mile wide, and is the geodiferous limestone. Wherever the upper surface of these is found polished, viz. over a considerable portion of several square miles, the polish is marked by striæ and furrows of very variable depth and width and distinctness. These have attracted some attention, and it may perhaps subserve the interests of geology, to give some of their directions and appearances, and refer to the cause of them.

The striæ and furrows are intermingled without any regularity. Sometimes several occur in an inch; at others, few or none in such a space; sometimes a dozen or more large and small in passing over eighteen or twenty inches. They are now very fine, now coarse; now shallow, then deep. Their directions vary considerably; some of them greatly. I have laid the compass upon many in many different localities, and taken the *bearing* with considerable care.

1. On an uncovered portion of the polished surface below the falls of the Genesee in this city, the directions of some are very nearly S. W.; of others, S. 56° W., S. 64° W., S. 22° W., and

S. 34° W. The angle at which these may cross each other, is readily seen. In the distance of a few feet, many of these different directions are parallel to each other. The same is true of the following.

2. At a mile east of these falls, in the same rock, the bearings are generally N. 22° W. and N. 8° E. Of the first direction, there are many, and of the latter there are few.

3. In an excavation made on Main street, the directions are S. 33° W. and S. W., and S. 56° W. I made many observations on these striæ and furrows. These three localities are on the east side of the river, and the first and third are near the banks of the Genesee.

4. At the rapids of the Genesee and near the south line of the city, many striæ are nearly southwest. The rock is here so finely polished, as to reflect light like a mirror, and is often in deep and uneven surfaces.

5. About one hundred rods west of the rapids, and just without the city limits, are a multitude of furrows and striæ. Some are S. W., some S. 22° W., and some are N. 8° E., while one is nearly N. The last shows its beginning and direction, and is the only one I have seen in which the course in which the power acted is evident. The beginning is a broken surface, as if a heavy rock had fallen upon the surface and crushed it in part, and then been moved northwards. This furrow is half an inch wide.

In some other places the directions are from southwest to northeast, or the reverse. In all these cases no allowance was made for the variation of the needle, about 5°. The earth over these surfaces before their exposure was from three or four feet to ten, and even from twenty five to thirty feet. In some instances the directions or bearings of a line changed a degree or two in the course of a few feet.

To R. W. Haskins, Esq. of Buffalo, I am indebted for the bearing of the grooves in the rocks in and about that city. The observations were made by himself and Dr. Hayes at Black Rock,* and in other places he was assisted by Mr. P. Sargeant; all well known to be highly intelligent, careful, and accurate observers. In and about Buffalo they have noticed eight or ten localities of

* See Dr. Hayes's remarks on the striæ in the vicinity of Buffalo, in this Journal, Vol. xxxv, p. 191.

the grooves, some in limestone, and others in shale. The former rocks are more or less polished, and the latter smoothed, while both are grooved. In the corniferous rock of Eaton, at Black Rock, the nodules of flint, being much harder than the limestone, stand up in bold relief, but are regularly smoothed like the other, and the surface gradually slopes on all sides of the nodule to the general level.

1. At Upper Black Rock the course of the grooves "was found by us, allowance having been made for magnetic variation, to be from N. 15° 32′ E. to S. 15° 32′ W." Twelve feet of earth had been removed from this surface.

2. At another quarry one mile and a half north northeast of the last locality, the direction was from N. 28° 12′ E. to S. 28° 12′ W.

3. At five miles southeast of Buffalo, on the banks of Buffalo Creek, in smoothed shale, they found the grooves with the same general direction from the north towards the south. Mr. Haskins makes the same remark on all the localities about the city and in it.

Causes.—From the multitude of bowlders of granite, mica slate, quartz, &c. there can be no doubt that a mighty current has swept from the north, and that these have been worn and rounded considerably by the attrition of the materials in the waters. But it cannot be supposed that such a cause alone could have *polished* the surfaces. It might smooth the rocks, as the current and some attrition now do in beds of the streams; but no polish is produced in that manner. After the surface had been polished, such a current moving along rocks or bowlders, might form the striæ, furrows and grooves.

The *glacial* theory offers a very simple and plausible solution of both the polish and the grooves. The whole process could be accomplished at one and the same time, as Agassiz and others state it now to be done under and by the glaciers in Switzerland.

The *motion* of a glacier by the expansion of the ice formed from the water which passes into the cracks and crevices, is most obvious, and entirely philosophical. One consideration, not adverted to distinctly by the writers on the glacial theory, makes the subject even more palpable. It removes too the necessity of supposing the passage of water into those minute undulating air-cells or cavities in the ice. It is this: *Ice receives its greatest*

expansion at the freezing point of water. The result of the crystalline arrangement of the particles then takes place. As the cold increases *the ice contracts*, like other bodies in the same condition. Hence it is that *ice cracks* on our rivers, ponds and lakes, as the cold becomes intense or is continued, and those cracks are from half an inch wide to a foot, and even four feet wide on the deep lakes. Hence the cracking of frozen ground when not covered with snow, with such tremendous explosions. Hence too the closing up of those cracks and rents as soon as the temperature rises near the point of congelation of water. The application of this principle is obvious. As the water trickles into the fissures in the ice of a glacier it must be frozen, and thus expanded; as the new ice cools more, contraction takes place, and new fissures or cracks must be the result. This process must be continually repeated in the warmer parts of the year, if the temperature of the ice below is depressed towards zero of Fahrenheit. Thus the heat of the atmosphere provides the trickling water, and congelation produces the motion by the expansion, and the cracks or fissures are renewed by the subsequent contraction. The expansion of the ice in different directions as it met with less obstacles would give striæ and grooves of various bearings. It is not certain the glaciers ever existed in this section of the country; it is certain that there was a rush of waters from the north.

The glacial hypothesis will not account for the position of the bowlders in many places. From the Alps a glacier may have transported the granite bowlders to a lower situation upon Mount Jura. But in our country we often find intervening hills higher than the source of the bowlders. The region south of Lake Ontario, which is covered with the sandstone bowlders from the shore of that lake, or a little south of the shore, is higher than the position of the sandstone. In Berkshire County, Mass., bowlders of graywacke, evidently removed from the hills of that rock in the adjoining part of the state of New York, are scattered through the valley of the Housatonic. But between the hills of graywacke and that valley, lies the Taconic range of mountains, along the boundary between the two states, and the latter are every where several hundred feet higher than the former. If the bowlders were once lodged on a glacier, the ice and bowlders must have been transported by a flood of waters over the

Taconic mountains. No glacier alone could have removed them to their present places, unless the Taconic range has since been elevated, or the graywacke hills have been depressed; and the evidence of either supposition is not to be seen in the dislocated appearance of the strata.

In Richmond, in Berkshire County, Dr. Reed has lately called my attention to a long line of bowlders of serpentine, stretching from the north part of Stockbridge across the valley of Richmond, and up a ravine on the Taconic range, and over that range into the valley beyond in the state of New York. Many of these are large bowlders, from twenty to fifty feet long, and twelve to twenty and forty feet wide, and eight to twelve feet thick. Of course, these bowlders show little appearance of having been rounded on their edges by attrition. Dr. Reed pointed me to one bowlder, fifty feet by forty, and twelve feet thick in some parts, and equivalent to forty feet square by ten feet deep, and containing near one thousand three hundred tons of two thousand pounds each. Several contain nearly one fourth as many tons. These bowlders occupy a space of only a few rods in width, while the graywacke bowlders are spread widely over the valley. The large bowlder is on the west side of the road leading from the church northwards, and some more than half a mile west of north from the church, on the eastern declivity of the Taconic range. These bowlders would seem to require, from their position and appearance, the combined machinery of large bodies of ice and of a mighty flow of waters from the west of north. It is only two or three miles north of the ravine just mentioned, that Prof. Hitchcock pointed out the furrows worn in the rocks by attrition and the mighty northern current, and at an elevation as high as that of the ravine.

Glaciers or icebergs and the strong current of waters, a union of the two powerful causes, probably offers the least objectionable solution of these wonderful changes, the polishing and furrowing of the rocks, and the transportation of bowlders. Indeed, the polishing might be done by great masses of floating ice in their mighty pressure upon the earth and gravel and stones upon the surface. If the motion of the glacier now polishes the rocks beneath, or even only smooths them, the motion of massive ice would effect the same, and also cut the striæ and grooves and furrows.

Rochester, N. Y., Sept. 17, 1842.

ART. XVI.—*Abstract of a Meteorological Register kept at Singapore, Lat.* 1° 20′ *North, Long.* 103° 52′ *East, from Nov.* 1, 1839, *to Feb.* 28, 1841, *with Notices of the Productions, &c. of the Island;* by JOSEPH S. TRAVELLI.

TO THE EDITORS.

Gentlemen—Herewith, in accordance with your request, I send an abstract of the meteorological register kept by me during the latter part of my residence in Singapore, together with some desultory remarks on the climate and on the state of agriculture on the island.

DATE.	THERMOMETER.					RAIN.					
	Highest of the month.	Lowest of the month.	Mean daily range.	Mean temp. from the highest and lowest of each day.	Mean from observations at 10 o'clock A. M. and 10 P. M.	Depth of rain, in inches.	Days on which it rained.	Hours of rain.	Evaporation.	Hours of sunshine.	Peals of thunder.
1839, Novem.,	87.30	73.00		78.72	78.76	11.17	21	62		123	67
" Decem.,	87.50	73.40	9.87	78.99	78.78	8.50	22	72	2.10	130	23
1840, January,	87.80	71.50	10.70	79.10	78.56	10.66	16	47	2.20	150	38
" Feb'ary,	87.20	71.50	11.59	78.99	78.84	6.45	23	45	2.60	178	180
" March,	88.60	73.00	10.60	79.84	80.60	6.64	15	25	2.35	207	103
" April,	89.20	73.70	9.87	80.33	80.31	10.37	18	61	2.30	198	171
" May,	90.00	73.00	9.77	80.76	81.01	5.16	14	36	3.20	221	74
" June,	88.00	72.00	7.96	81.16	81.34	7.97	9	43	3.45	211	18
" July,	88.70	72.00	9.79	80.86	81.40	5.56	10	20	4.75	228	42
" August,	88.00	71.80	8.34	79.59	80.54	8.66	13	46	3.85	174	72
" Septem.,	89.00	72.40	8.65	79.88	79.60	8.00	12	31	4.00	147	19
" October,	88.60	72.00	9.44	80.46	80.30	8.80	15	36	4.00	162	167
" Novem.,	86.00	73.00	7.75	81.12	78.54	7.27	16	47	3.40	79	3
" Decem.,	86.30	71.80	8.63	78.55	79.76	7.82	18	57	2.90	99	163
1841, January,	87.80	71.00	9.85	77.81	78.04	7.29	13				
" Feb'ary,	88.00	68.70	11.30	79.80	80.53	2.50	5	6		152	2

The observations were made at the mission school of the American Board of Foreign Missions, at Ryan's Hill, about half a mile from the town. The hill is about one hundred or one hundred and twenty feet above the level of the sea. The register from which the abstract is made, includes hourly observations, from half an hour or more before sunrise to ten o'clock, P. M., with occasional observations through the night, for almost every day of the sixteen months mentioned in the abstract. These

details, however, would hardly suit your Journal, and I have therefore limited the abstract to the items of more general interest.

The statement as to the hours of rain, and the hours of sunshine, are of course mere approximations. In regard to the thunder, the difficulty is still greater. Some would understand "a peal of thunder" one way, and some another. My object was to record the number of times it thundered, without reference to the loudness of the report. In many instances I should, perhaps, have conveyed my impressions better by calling them a series of rumblings rather than distinct peals. We very seldom had what would be called in the United States a heavy thunder storm. In regard to the hours of rain and sunshine, and peals of thunder, the register pretends to nothing more than *guessing*. And yet to some, on these points, the mere *guess* of a person on the spot, recorded from day to day, for several months, may be interesting. The thermometer was placed in an open veranda, to which the external air had free access at all times. The cup for evaporation was suspended by the side of the thermometer.

The observations extend from Nov. 1, 1839, to Feb. 28, 1841. Taking the year 1840 by itself, the register gives the following results for the year.

Mean temperature,	80.09° Fah.
Highest "	90.00
Lowest "	71.50
Mean daily range,	9.34
Whole range,	18.50
Rain, (of which $\frac{5}{12}$ fell between 6 and 12 A. M.)	93.36 inches.
Evaporation,	39.00 "
Days on which it rained,	189
Hours of rain,	494
Hours of sunshine,	2054*
Rumblings or peals (?) of thunder,	1050

The climate of Singapore is an exceedingly pleasant one. The frequent rains throughout the whole course of the year, together with the constant sea breezes, free it in a great measure from the sultry heat of most tropical countries. The general distribution of rain throughout the year, has the happiest effect on vegetation, keeping it constantly fresh, and green. There is no rainy season

* Making an average of nearly six hours of sunshine for each day in the year.

in the ordinary sense of the word, although there is a manifest difference between the months corresponding to our winter and summer months, the winter months being more wet and the summer months more dry than at other times.

It is now about twenty three years since the island was settled by the British, and the population already amounts to about forty thousand. This population is composed chiefly of Malays and Chinese. The settlement is in every respect in a thriving condition. New roads are making around the island and into the interior. New streets are opening and new buildings going up in all parts of the town. In some directions as you ride along, you might imagine yourself in one of our new western settlements, from the vigor with which the agriculturalist is clearing and cultivating the land.

The investment of capital in agriculture, by Europeans, was retarded for a long time by the uncertainty which existed as to the terms on which the government would sell or let out the land. But this difficulty has been removed to a great extent within two or three years, by the adoption of definite regulations on the subject, putting the land at low rents on long leases. None of the land is sold by the government, in fee simple, but is let out on leases for a term of years.

Nutmegs, coffee and sugar are the principal products of the plantations of Europeans, while gambir, pepper, siri, betel-nut, and vegetables, are the products of the native farms. The clove was formerly cultivated with the nutmeg, but it is not much attended to now. It is regarded as more troublesome, less productive for the time being, and less durable than the nutmeg.

The *nutmeg* is usually planted on the hills or rising grounds in the vicinity of the town. Being a handsome tree, and usually set out with great regularity and carefully cultivated, it is continually adding to the beauty of the place. The trees, if well attended to, usually begin to bear fruit pretty well by the seventh or eighth year, and are said to go on increasing in value for many years. After the trees begin to bear well, say by the tenth year, they are said to yield a very large return on the capital invested. It is also said that should the price of nutmegs in commerce diminish one fourth, or even one third, that they would still afford a fair return. The trees are generally set out from twenty to thirty feet apart in the quincunx order, with the view of hav-

ing one male tree in the centre of the largest number of female trees. It is not usual to set the trees out in this way, until they are from three to five or even six years old. Previous to this they remain in the nursery. The nuts are planted in the nursery from six to twelve inches apart. When the plants are about two years old, they are usually transplanted into another nursery, where they remain until they are set out as above mentioned. The young trees however, are said to be improved by transplanting, and some accordingly transplant them once a year before finally setting them out. When the nuts are first planted, they must be separated from the thick hull or rind which envelopes them, and they must be planted within twenty-four hours from the time they are taken from the tree. It is usually six weeks or two months before they come up. Some persons prefer keeping the trees in the nursery until they have blossomed, and thus shown their sex, previous to finally setting them out. When they are set out before they have shown their sex, it is always at the risk of a considerable amount of labor and expense for nothing, as the number of male and female trees is about equal, and as among the young trees there is no way of distinguishing the sex until they put forth their blossoms, which seldom appear before the fifth year.

The holes into which the trees are set at last, are usually prepared several months before the time of transplanting. Each hole is about five feet deep, and about five in diameter. This hole is filled with vegetable matter, top soil, and manure. As the mass settles down it is refilled in a similar manner. A sort of shed about seven feet high, open on two sides, is then built over each hole, and is covered on the top and on the two sides with a sort of thatch made of the leaves of the Nipah palm, (*Nipah fruticans*, Thunberg.) This shed lasts two or three years. By this time the tree has attained a sufficient size, and is left without any further protection. The tree grows slowly, but attains in time quite a large size. Its size and productiveness depend chiefly on the mode of cultivation. No grass or weeds are to be left about the roots, and suckers are to be removed. Every two or three years the tree is to be thoroughly manured. This is done by making a trench about a foot or eighteen inches deep, and as many in width, entirely around the tree, and corresponding in distance from the trunk with the outermost branches. This

trench is filled with grass, top soil, manure, &c. as before. The object is to get the manure near to the ends of the roots, and this is effectually accomplished by this plan. One might think that the tree would be injured by wounding the roots, but constant experience shows that nothing tends so much to the health and vigor of the tree as this manuring. When digging the trench, in case they discover signs of disease in the root they do not hesitate to cut in to within a foot of the trunk even of a large tree, cutting off in so doing, roots two or three inches in diameter. This is frequently done also in getting out the roots of a species of grass which is the pest of the agriculturist in that part of the world. It is called by the Malays "sallang," (*Andropogon caricosum*, Lin.) It is a long grass, sometimes four feet or more high. It frequently covers whole fields, rendering them comparatively valueless from the difficulty and expense which it would cost to get out this grass. In the rich beds of the nutmeg it grows with great rapidity and must therefore be got out at all hazards. Its tenacity of life is so great that there is no security against its growing again, but by burning every piece of the root. Exposure of the root to the sun for months seems not to destroy its vitality entirely. And I have known it to come up through a layer of earth and gravel four feet deep, thrown out in digging a a cellar.

The foliage of the nutmeg being thick and of a dark rich green color, it is quite an ornamental tree. The brilliant colors of the ripe fruit, as the bright scarlet of the mace and the glossy black of the shell under the mace are seen through the bursting rind which evinces the maturity of the fruit, make it one of the most beautiful products of the vegetable world.

There is no particular season for gathering the fruit, though I have seen the contrary stated in some works on botany. The usual practice is to go around the plantation every day to examine each tree in quest of ripe nuts. Almost every good tree will exhibit more or less of ripe or unripe nuts, as well as buds and blossoms, nearly every day of the year. It is nevertheless true, that at certain times particular trees will bear more than at other times, and even particular plantations more than other plantations in the same neighborhood, but as far as I could learn from observation or by enquiring of those practically engaged in the business, there is no such thing as a nutmeg season.

Coffee. This has not been cultivated to any extent in Singapore until quite recently. As the nutmeg trees require so much room when they attain their full size, and as for some fifteen or eighteen years there is a large space left between the rows, they frequently plant coffee trees between the rows of nutmeg trees. As the coffee trees die out in the course of fifteen years, they do not interfere with the nutmegs, and yet occupy the ground to good advantage in the mean time. They are seldom over eight feet high. The plants are raised in the nursery, and are set out in holes, somewhat as described in reference to nutmegs, except that the holes and sheds are very small.

Sugar. The sugar cane has long been grown in the island in its natural state, for the use of the natives. But the cultivation of it on an extensive scale for the purpose of manufacturing sugar was introduced by our enterprising, polite and hospitable consul, J. Balestier, Esq. Mr. B. after having overcome a great variety of obstacles is now succeeding well. He obtained some time ago a gold medal from the Agricultural Society of India for the best sugar manufactured in the British East India possessions. His success is stimulating others to engage in the same business.

Of the products of native cultivation, pepper and gambir are the chief. The two are usually cultivated together, as the leaves of the gambir plant are supposed to form a very good manure for the pepper vines.

Pepper (*Piper nigrum*) is usually planted three or four feet apart, with stout rough rails, stuck into the ground, and eight or nine feet high, on which the vines climb. The pepper gardens resemble patches of hops, except that the foliage is thicker and of a darker green.

Gambir appears to be the produce of two different plants, one one of them a climbing plant, (*Nauclea gambir,*) and the other a shrubby plant. That from which this article is obtained in Singapore, is a shrubby plant from five to six feet high. The leaves and twigs are broken off and boiled in water in a large kettle or boiler until their strength is extracted, when the leaves are taken out and strewed as manure around the pepper and siri vines. The extract is then evaporated until it becomes like a thick stiff jelly. It is, probably, sometimes thickened also with earth and sago. While in this jelly state it is cut into cubes of about an inch, or a little more. It is sometimes called catechu, and also

Terra Japonica, in the books, and is also sometimes confounded with kino. Some of the books speak of kino and gambir as the product of the *Nauclea gambir*, which is doubtless one of the plants above alluded to. This article is used very extensively throughout India, and the Malay islands, as an ingredient in the masticatory or *quid*, in which the natives of all ranks indulge. This masticatory consists of gambir, betel-nut, tobacco, shell-lime and siri leaf, a green, pungent leaf in which the other ingredients are wrapped. Gambir has latterly become a considerable article of export to England, where it is used extensively for tanning purposes, on account of the large quantity of tannin which it contains. One pound of gambir is said to be equivalent, for tanning leather, to seven or eight pounds of oak bark. It is usually sold in Singapore at $2 50 to $2 75 per picul of one hundred and thirty three pounds.

Siri (*Piper betel*) is cultivated in gardens in the same way with pepper. It is a vine, belonging to the pepper family and resembling the *Piper nigrum*. It is cultivated altogether for the use of the natives. The leaf is the part principally used, although the fruit, somewhat resembling the spike or ear on which the pepper grows, is sometimes chewed by the natives, as a part of their *quid*.

Vegetables.—Of such as are found in the United States, the following are to be found in the Singapore market every day. Radishes, lettuce, pumpkins, squashes, cucumbers, egg-plants, (*Solanum melongena*, L.) sweet potatoes, onions, celery, string beans. Watermelons and musk-melons are also frequently to be had, but of a very inferior quality.

The chief native laborers by whom these articles are raised, are the Chinese. There are a large number of emigrants from China, arriving every year. The government encourage their going into the jungle as it is termed, and "squatting," and they will not allow them to be ousted without an equivalent for any improvements or clearings they may have made. As they pay no rent, and as there is plenty of excellent land to be found, and as they find ready sale for whatever thay can raise, they generally do well, especially as they are proverbially industrious.

Allegheny City, Pa., March 10, 1842.

ART. XVII.—*Abstract of the Proceedings of the Twelfth Meeting of the British Association for the Advancement of Science. Condensed from the Report in the London Athenæum.*

Continued from Vol. XLIII, p. 376.

Sir J. Herschel stated, that the committee for *revising the nomenclature of the stars*, deferred reporting till the catalogue of stars, now in preparation under the auspices of the Astronomical Society, was ready for publication; and that the committee for the *reduction of meteorological observations*, in consequence of the illness of Mr. Birt, had been unable to make any considerable progress.

Sir D. Brewster gave a provisional report respecting the erection of one of *Mr. Osler's anemometers*, at Inverness. He also made a provisional report on the *hourly series of meteorological observations* made at Inverness from Nov. 1, 1840, to Nov. 1, 1841. The mean temperature of Inverness for the summer months was 52°.258; the mean temperature of the winter months, 40°.287; and the mean temperature of the whole year, 46°.272. This mean temperature occurred at 8h. 33m. A. M. and 7h. 42m., the critical interval being 11h. 9m., differing only a few minutes from the result obtained by similar hourly observations made at Leith. The observations made with the barometer, when reduced to the level of the sea, and to the temperature of 60°, indicate very distinctly the daily variation, with its two maxima and minima. The mean annual average of all the observations was 29.680 inches. The monthly mean indicated a *maximum* in December and in June, and a *minimum* in March and October.

Prof. Phillips called attention to the remarkable character of the curves of barometric oscillation which appeared derivable from these observations. As he understood the statement, the hours of maximum and minimum were found to be very different from, and even in contrast with those at which the oscillations occurred further south, as for example, about London. He pointed out the support which these observations lend to the formulæ of barometric oscillations in various latitudes and at different elevations, given in the Edinburgh Philosophical Transactions.

Mr. Scott Russell presented a report on the *abnormal tides of the Frith of Forth*, containing the results of his very elaborate

observations. He also communicated a supplement to the former Report of the *Committee on Waves.* He divided waves into three great orders, obeying different laws. 1. Wave of the first order,—the wave of translation,—solitary, progressive, depending chiefly on the depth of the fluid: of two species, positive and negative. 2. Waves of the second order,—oscillatory waves,—gregarious: the time of oscillation depending on the amplitude of the wave. 3. Waves of the third order: capillary waves,—gregarious. The oscillations of the superficial film of a fluid, under the influence of the capillary forces, extending to a very minute depth: short in duration: of two species,—free and constrained. The various phenomena of these classes of waves had been thoroughly examined by the committee, and many interesting results developed.

*On a very curious fact connected with Photography, discovered by M. Möser, of Königsberg,** communicated by Prof. Bessel to Sir D. Brewster. The following are the general facts connected with the discovery. A black plate of horn or agate is placed below a polished surface of silver at the distance of one twentieth of an inch, and remains there for ten minutes. The surface of the silver receives an impression of the figure, writing or crest, which may be cut upon the agate or horn. The figures do not appear on the silver at the expiration of the ten minutes, but are rendered visible by exposing the silver plate to vapor, either of amber, water, mercury, or any other fluid. He (Sir D. Brewster) had heard Prof. Bessel say, that the vapors of different fluids were analogous to the different colored rays of the spectrum; that the different fluids had different effects, corresponding to those of the spectrum; and that they could, in consequence of such correspondence, produce a red, blue, or violet color. The image of the camera obscura might be projected on any surface —glass, silver, or the smooth leather cover of a book—without any previous preparation; and the effects would be the same as those produced on a silver plate covered with iodine.

This paper gave rise to an animated conversation, in the course of which M. Bessel said that he had seen some of the pictures taken by this process, which were nearly, but not quite, as good

* See Dr. Draper's letter of reclamation of this discovery, at page 203 of this number.

as those obtained by Mr. Talbot's process. Sir D. Brewster said, this was the germ of one of the most extraordinary discoveries of modern days; by it there seemed to be some thermal effect which became fixed in the black substance; and not only so, but M. Bessel informed him, that different lights seemed to affect different vapors variously, so that there seemed to be something like a power of rendering light latent; a circumstance, which if it turned out so, would open up very new and curious conceptions of the physical nature of light: on the emission theory, it would be easy to account for this;—on the undulatory theory, he could not conceive how it could be possible.

Prof. Bessel, of Königsberg, made a communication on *the Astronomical Clock*. Having ever been of opinion, that this indispensable instrument to the astronomer, the transit clock, could only acquire perfection, if the pendulum, (separated from the works,) were made to vibrate in equal time, whatever the temperature and the arc might be: he would submit, whether the expeditious method of coincidences might not be employed for checking the pendulum in both respects. The pendulum, apart from the clock, being suspended from the wall; a clock, taken out of its case, might be placed before it, at a distance of six or eight feet; an object-glass of three or four feet focal length might be placed between both, so as to produce, exactly at the lower end of the pendulum of the clock, an image of the lower end of the other pendulum. Then the coincidences of both might be accurately observed by a telescope placed at a convenient distance. Similar contrivances had been described in an account of some pendulum experiments made at Königsberg: and the accuracy of the method was such, that the relative rate of both pendulums might be ascertained with sufficient accuracy in a very short time,—in from ten to twenty minutes. The rate of the pendulum was to be tried at different temperatures, being placed in a box, having an opening at the lower end, covered with glass, and so fastened to the wall, that the pendulum could swing within it. In the construction of the pendulum, attention should be paid to one thing, which seemed to have been much overlooked. It often happened that thermometers affixed to the top and bottom of a clock case did not agree, whence it was evident that the compensation, acting only below, would not compensate for the variation of the whole rod. He should prefer, on this

account, the gridiron to the mercurial pendulum, especially if the rods began as low as possible below the point of suspension, and were carried on to the centre of gravity of the lens. He should prefer the several rods to be of equal diameter, and to be coated uniformly. Supposing the spring perfectly regulated, as well with respect to heat as the arc, only one cause would interfere with regular vibration-times. This was the effect of that part of the elasticity of the air which depends on the variation of the height of the barometer:—the other part, depending upon the variations of the thermometer, is comprised in the adjustment for the compensation for heat. There was a possibility of compensating the former, by fastening a barometer tube to the pendulum, and it would not be difficult to find the suitable diameter of the tube: but this complication of the pendulum would be rather inconvenient. At all events, the variations of the barometer were not very great, especially if the specific gravity of the pendulum be made as great as possible.

Application of the principle of the Vernier to the subdividing of Time, by Mr. F. Osler. A pendulum is to be used which should make ten swings in the time that the principal pendulum makes eleven, furnished with a small dial, and so placed that the coincidences or want of coincidence could be observed. The strokes of such a pendulum being counted, the time of every observed stroke of it, reckoned back from its coincidence with the principal, or seconds pendulum, would, it is obvious, be found in tenths of a second.

Mr. Dent reported on his *chronometrical experiment to determine the difference of meridians* between Greenwich and Devonport. He also reported respecting his *steel Balance Spring coated with pure gold* by the electro-metallurgic process; also of the performance of his clock, in which the impulse is given to the pendulum at or near the centre of percussion. By this contrivance he purposed to obviate the difficulty occasioned by the freezing of the oil at low temperatures. The stopping of clocks at very low temperatures had induced the Astronomer Royal to invent a *new* escapement which seemed to answer all the conditions required; an addition of twelve pounds could be added on to the weight of the clock, and yet a variation was produced in the arc of vibration amounting to only five minutes, while an addition of one pound to the weight of the ordinary Graham's

escapement, made a difference of fifteen minutes. By Mr. Airy's plan, there was always (if the term may be used) an extra reservoir of force; keeping the train of wheels always up to their work, and capable of overcoming the resistance occasioned by the freezing of the oil. Mr. Dent then explained the principle of his patent Compensation-balance. Mr. Frodsham made some remarks on the compensation balance of chronometers, and explained a new balance of his invention. Sir Thomas Brisbane said, that praise was due to Mr. Dent as the first maker who had exerted himself to determine the difference of meridians by chronometers. He had shown that by chronometers the difference of longitude could be had with as much certainty as by any other method in use, and at an expense bearing no proportion to that of rockets, or any other means hitherto adopted. Dr. Robinson, of Armagh, was at present engaged in a series of rocket observations in Ireland. It had been the intention of Dr. R. to connect the Irish with the Scotch observatories, and for that purpose a large deposit of rockets had been obtained from government, and stood in Dumbarton Castle; but unfortunately the unfavorable weather in spring had prevented the execution of the design, and he had received a letter within a few days from Dr. Robinson, stating that the strong twilights of the present season would make it requisite to postpone the work until autumn: these facts would at once show the superior economy and saving of time to be attained by adopting Mr. Dent's suggestion of chronometrical observations. Mr. Holden inquired, why the method of moon-culminating stars, which was so simple and easy of application, was not preferred to any other in determining longitudes. Sir Thos. Brisbane replied, that not to say nothing of the heavier amount of labor required in such observations, he need only, in order to show the superiority of Mr. Dent's method, state the fact, that in a late attempt to connect the Royal Observatories of London and Paris, backed by all the instrumental accuracy and unrivalled skill of the observers at these two distinguished observatories, three hundred observations on moon culminating stars had given a mean deviating no less than thirty seconds from the truth. The President observed, that although the method of moon-culminating stars had in theory promised considerable accuracy in the determinations of longitudes, yet from some unexplained difficulties, it had in practice fallen far below the estimate that had been formed of it.

Sir D. Brewster made several communications. 1. On Luminous Lines in certain flames corresponding to the defective lines in the Sun's light. 2. On the structure of a part of the Solar Spectrum hitherto unexamined. 3. On the luminous bands in the Spectra of various flames. I. After noticing Fraunhofer's beautiful discovery as to the phenomena of the line D in the prismatic spectrum, Sir David said, he had received from the establishment of that eminent man, at Munich, a splendid prism, made for the British Association, and one of the largest perhaps ever made; and upon examining by it the spectrum of deflagrating nitre, he was surprised to find the red ray, discovered by Mr. Fox Talbot, accompanied by several other rays, and that this extreme red ray occupied the exact place of the line A in Fraunhofer's spectrum, and equally surprised to see a luminous line, corresponding with the line B of Fraunhofer. In fact all the black lines of Fraunhofer were depicted in the spectrum in brilliant red light. The lines A and B turned out in the spectrum of deflagrating nitre to be both double lines; and upon examining a solar spectrum under favorable circumstances, he found bands corresponding to these double lines. He had looked with great anxiety to see if there was any thing analogous in other flames, and it would appear that this was a property which belonged to almost every other flame. II. He had, by means of the prism from Munich, been enabled to extend the solar spectrum beyond the point where, according to Fraunhofer, it terminated immediately at the side of the line A, and he (Sir David) found one part to consist of about sixteen lines, placed so near to each other, that it was difficult to recognize the separation; but the lines as they approached to A were much nearer to each other than as they receded from it; consequently, that portion of the spectrum appeared concave, resembling so much the scooped-out lines of a moulding on wood, that it was scarcely possible to suppose that the beholder was not looking at such a moulding. He was led to observe an analogous structure near the line B; and upon carrying on this comparison of structure of one part of the spectrum with that of another, it seemed to him, that by and by, something important would result: for there was a repetition of a group of lines, and similar lines, through different parts of the spectrum, as if the same cause which produced them in one part, produced them in another. III. He had endeavored to procure all the minerals and artificial salts and other substances

capable of combustion which could be had; and in order to have a suitable combination, he used an oxygen light analogous to the Bude light. Every one conducting these experiments was aware that it was necessary to pass the light through a narrow aperture; but this would reduce the intensity of the light so much, as to make it difficult to observe the rays at the extremity of the spectrum; but he found that he could obtain the effect of a small aperture by merely inclining the prism; so that with a good prism, the great lines in the solar spectrum might be seen by using an aperture three or four feet wide, the whole breadth of the window, by the mere inclination of the prism, which had the effect of producing a narrowing, facing the light. He had obtained two hundred or three hundred results which he had not had any leisure to group; but he would mention some of the general results. When nitrate of lead was thrown into combustion, remarkably fine lines were produced in the spectrum. The luminous line D of Fraunhofer, existed in almost every substance, especially in all into which soda entered, particularly in the flame of a common tallow candle: probably owing to the muriate of soda existing in the tallow. The hydrate of strontia and the iodide of mercury gave the lines very remarkably in yellow and green. Also in that remarkable substance, the lithoxanthemate of ammonia, first discovered and published by Mr. Fox Talbot, the fine lines were seen throughout the whole length of the spectrum; and there was a remarkable blue band, which he (Sir D.) had not distinctly recognized in any other flame. Indigo gave fine green and orange lines at equal distances from the D of Fraunhofer. Prussian blue did the same: calomel, nitrate of magnesia, litharge, also showed lines; the sulphocyanite of potash gave a violet and orange flame, with the lines extremely distinct. He hoped at the next year's meeting of the Association to be able to embody these various results in a regular report.

Sir D. Brewster made a communication on a *New Property of the rays of the Spectrum, with observations on the explanation of it given by the Astronomer Royal, on the principles of the Undulatory Theory.* If we cover half the pupil of the eye with a thin plate of any transparent body, and thus view a prismatic spectrum, so that the rays which pass by the plate interfere with those that pass through it, the spectrum is seen crossed with beautiful black and nearly equidistant bands, whose breadth, generally speaking, increased with the thinness of the plate. If

the edge dividing the ray were directed to the red end of the spectrum, then fringes were seen: but no such fringes appeared when it was turned to the violet end of the spectrum. One peculiarity of these fringes, not before noticed, was that they had not the forms of bands, but rather the appearance of screws, or dotted black lines, or as if they were formed by the shadow of a plate of metal perforated by small openings. This, which appeared to be a new property of light, and to indicate a polarity in the simple rays of light, when separated from each other by refraction, he had commented on at the meetings of the Association at Liverpool and Bristol: and Mr. Airy, the Astronomer Royal, had given a paper and two publications on the subject, in which he endeavored to account for this upon the undulatory theory, arguing that the apppearance and magnitude of the fringe depended upon the diameter of the pupil or of the object-glass. Sir D. Brewster said, he had repeated all his experiments under every variety of form, varying the diameter of the pupil from its greatest expansion to its greatest contraction, and the diameter of the object-glass from four inches to a quarter of an inch, and the fringe remained utterly unaffected by these variations. He further found, that these fringes varied in magnitude with the distance of the eye from the refracting body, and not with the magnitude of the pupil. He stated several other results, all of which, he thought, could not be explained on the principles of the undulatory theory. This paper gave rise to much discussion; the prevailing opinion appearing to be that future discoveries would probably reconcile the phenomena in question with the wave-theory of light.

Sir D. Brewster made a communication on the *Dichroism of the Palladio-chlorides of Potassium and Ammonium.* Dr. Wollaston had found that a long crystal of either of these salts, when looked through transversely, had a green color, but when looked through from either end, had a red color; and he (Sir David) placed one of these long crystals transversely over another, in a cruciform shape, and then found that those portions of the centres of both, which were in contact, gave a red color, while all the ends of the crystals were red.

On the Geometric forms, and laws of illumination of the spaces which receive the solar rays, transmitted through quadrangular apertures; by Sir D. Brewster. His attention was called to

this subject by an accidental discussion on the point whether or not Aristotle, in explaining the circularity of images formed by quadrilateral apertures, employed the appropriate idea when he said that those images are to a certain extent quadrilateral, but appear circular, because the eye is unable to recognize faint impressions of light. Prof. Whewell, in his History of the Inductive Sciences, had distinctly stated that Aristotle had not used the appropriate idea, and that the question was entirely a geometrical one, the appropriate idea being the rectilinear nature of light. Having been accidentally led to consider the subject, he (Sir David) had determined in a simple manner, the form of the aperture at all distances, had been led to take the same view of the subject with Aristotle, who seemed to have employed the appropriate idea.

Papers by Sir D. Brewster were also read on *Crystalline Reflection;* and on *the Existence of a New Neutral Point and two Secondary Neutral Points.*

On the improvement of the Telescope; by Mr. Fox Talbot. Mr. T. said that this subject occurred to him about two years ago, when the Earl of Ross (then Lord Oxmantoun) was making much larger specula for reflecting telescopes than had ever been obtained before: and he thought, if once we had a very large and perfect speculum, it might be possible to multiply copies of it by galvanic means. He had observed, that if an electrotype cast were taken from a perfectly polished surface, the cast was also perfectly polished; so that no defect of form from this cause could have an injurious effect on the speculum. The great and obvious defect was, that electrotypes were in copper, which reflects but little light. He mentioned these ideas to Professor Wheatstone, who said the same had occurred to him: and he showed him a paper which he had drawn up some few months before, and in which he suggested the taking galvano-plastic casts of specula in platina, palladium, silver or nickel, and for especial purposes gilding the copper; taking care that the two precipitations adhered well to each other. So that, said Mr. T., the idea had suggested itself independently to both of them; but on comparing notes, they found differences. Though it had occurred to Mr. T. to precipitate white metals, yet he did not think that platina would have a sufficiently beautiful white metallic polish. Prof. Wheatstone had, however, made choice of platina;

and varying the quantity till he found the required proportion, he obtained a mirror in platina, which appeared to have quite brilliant polish enough, and to be white enough to answer the purpose; and he considered, therefore, that Prof. W. had proved that at least in one form the specula of telescopes might be made by voltaic precipitation. His own idea was, that it might be possible to whiten the surface of the copper without injuring the form; and therefore, having obtained a speculum in very bright polish, he (Mr. T) whitened it and transformed it into sulphuret of copper: and after having retained it about a year, he did not perceive the smallest alteration in any respect. This therefore appeared to him a mode by which important results for astronomers could be obtained. For the last year, perhaps, nothing further had been done, either by Prof. Wheatstone or himself; but the other day, being at Munich, he (Mr. T.) visited Prof. Steinheil, who showed him his inventions, and told him he had discovered a method of making specula by the electrotype. It so happened, that Prof. S. and himself had published their respective methods about a month or six weeks before: the Professor having read a communication on the subject before the Academy of Sciences at Munich, and printed it, and he (Mr. T.) having published his in England. Their modes were, however, different, as Prof. S. precipitated gold upon the speculum of copper; and having precipitated a certain thickness of gold, he then precipitated copper on the back of the gold, to give it sufficient thickness. He (Mr. T.) should have thought beforehand that gold would not reflect light enough to be available; but Prof. S. informed him he had found, by careful experiment, that it reflected more light than polished steel. He allowed Mr. T. to look through a Gregorian reflecting telescope, of which the speculum was a common one but gilded, and he found, although a slight tinge of yellow was thrown over all the objects, the image was perfectly clear and well defined. Prof. S. said that in the course of the year, he should have a very large telescope, furnished not only with a speculum, but also with other apparatus, voltaically formed, so that telescopes might all be made from a good model, so as to insure greater accuracy of proportions: and in this way even very large telescopes might be constructed at a comparatively trifling expense. With reference to precipitating copper on the back of the gold, the Professor had a simple expe-

dient for securing adhesion. He first precipitated gold from the cyanide of gold, and he mixed with it cyanide of copper, and kept gradually increasing the quantity of the latter sort; so that an alloy was precipitated, which was continually increasing the copper with respect to the gold, till he had a speculum whose surface was gold, and which then became an alloy, the quality decreasing, till at the bottom it became pure copper. This was important: because, without such experiments, one would not have known that such results would have followed; for some philosophers supposed, that if we attempt to precipitate the salts of two metals, only one was precipitated; but Prof. S. informed him that they precipitated in union. He thus obtained a speculum with a face of gold and a back of copper. But supposing the largest, cheapest, and best speculum were obtained, the framework of the telescope would be so gigantic, that few observers would be able to use the instrument. With a focal length of sixty to eighty feet, it would be quite unmanageable for any private individual. The idea occurred to him (Mr. T.) to have a tube fixed in an invariable position, and to have a perfectly true plane mirror, of a size somewhat larger than the concave speculum, placed in front of the tube, with an aperture in the centre. This plane reflector should be movable about its centre in any direction: so that luminous bodies, falling first upon the plane reflector, were then reflected against the concave reflector and passed through the aperture. The only motion requisite for the plane mirror would be one about its centre. The mechanical difficulties in the way of this plan would be far less than in the common method. Prof. S.'s idea on this point was somewhat different. He (Mr. T.) did not think important in what direction the tube of the telescope was placed. Prof. S.'s idea was, that it should be pointed directly to the pole of the heavens, and kept as steady as possible, and that the plane mirror should have a simple motion of revolution, indeed two motions, but about a rectangular centre.

Experimental inquiries on the strength of stones and other materials; by Mr. Eaton Hodgkinson. Numerous experiments on wood, sandstones, marbles, glass, slate, ivory, bone, &c. had been made by Mr. H. in order to ascertain the tensile, crushing, and transverse strength of each; also, as far as possible, the situation of the neutral line. The following is a summary of the

results obtained on marbles and stones of various degrees of hardness.

Description of stone.	Crushing force per square inch called 1000.	Tensile force per square inch.	Transverse strength of bar 1 inch square and 1 foot long.
Black marble,	1000	143	10.1
Italian marble,	1000	84	10.6
Rochdale flagstone,	1000	104	9.9
High Moor stone,	1000	100	
Stone called Yorkshire flag,	1000		9.5
Stone from little Hulton,	1000	70	8.8
Mean rates,	1000	100	9.8

Or calling the mean crushing strength per square inch in the different articles experimented upon 1000, we have

	Crushing strength 1000.	Tensile strength.	Transverse strength.	Ratio of mean tensile to crushing strength.
In timber,	1000	1900	85.1	1 to 0.55
Cast iron,	1000	158	19.8	1 " 6.6
Glass, plate and crown,	1000	123	10.	1 " 7.8
Stone and marble,	1000	100	9.8	1 " 10.5

The ratio of the crushing force to the transverse force is nearly the same in glass, stone and marble, including the hardest and the softest kinds. Hence if we know the transverse strength, in any of these bodies, we may predict the other; and as glass and the hardest stones resist crushing with from seven to nine times the energy that they do being torn asunder, we may get an approximate value of the tensile force from the crushing force, or *vice versa*. These results render it probable that the hardest bodies, whether cast iron, glass, stone or marble, admit of certain atomic displacements, either in tearing asunder or crushing; these displacements being in a given ratio to each other or nearly so. In future calculations as to the strength of bodies, the crushing strength ought to be made the fundamental datum, for the reasons shown in this notice. The ratio of the transverse strength to the crushing strength is greater in cast iron than in glass, marble and sandstones, arising from the ductility of that metal. The necessity of enlarged inquiries in these matters will be seen, when it is reflected that calculations of the tensile strength of cast iron or marble or stones in general, made from the transverse strength by the modes used by Tredgold, Navier and others, give the tensile strength twice or three times as great as it ought to be.

Mr. Hodgkinson also presented a communication on the mode of conducting experiments on the resistance of air.

On a new general principle of Analytic Mechanics, by Prof. Jacobi of Königsberg. In the different problems relative to the motion of a system of material points which have been hitherto considered, one may make, said the Professor, an important and curious remark, "that whenever the forces are functions of the coördinates of the moving points only, and the problem is reduced to the integration of a differential equation of the first order of two variables, it may also be reduced to quadratures." The author has succeeded in proving the general truth of this remark, which appears to constitute a new principle of mechanics. This principle, as well as the other general principles of mechanics, makes known an integral, but with this difference, that whilst the latter give the first integrals of the dynamical differential equations, the new principle gives the last. It possesses a generality very superior to that of other known principles, inasmuch as the analytical expressions of the forces, as well as the equations by which we express the nature of the system, are composed of the coördinates of the movables in any manner whatever. The Professor proceeded to show the application of his principle, and its advantages in the following problems, viz. the orbit described by a planet in its motion round the sun; the motion of a point attracted to two centres of force, after Newton's law of gravitation; and the problem of the rotatory movement of solid bodies round a fixed point. He then enunciated the rule itself, by which the last integration to be effected in the problems of mechanics, is found to be reduced to quadratures, the forces being always functions of the coördinates alone; and observed, that when we have any system whatsoever of material points, the simplicity of the preceding theorem is in no respect altered, provided we give to the dynamical differential equations, that remarkable form under which they have been presented for the first time by the illustrious Astronomer Royal of Dublin, and under which they ought to be presented hereafter in all the general researches of analytical mechanics. It is true that the formulas of Sir W. Hamilton are referable only to the cases where the components of the forces are the partial differences of the same function of the coördinates; but it has not been found to be difficult to make the changes which are necessary, in order that these formulas may become applicable to the general case

where the forces are any functions whatsoever of the coördinates. When the time enters explicitly into the analytical expressions for the forces, and into the equations of condition of the system, the principle of the final multiplier found by a general rule, is applicable also to this class of dynamical problems. There are also some particular problems into which enters the resistance of a medium, which give rise to similar theorems. It is the case of a planet revolving round the sun in a medium whose resistance is proportional to any power of the velocity of the planet. "The analysis," observed M. Jacobi, "which has conducted me to the new general principle of analytical mechanics, which I have the honor to communicate to the Association, may be applied to a great number of questions in the integral calculus. I have collected these different applications in a very extensive memoir, which I hope to publish on my return to Königsberg, and which I shall have the honor of presenting to the Association as soon as it shall be printed."

On certain cases of Elliptically Polarized Light, by Prof. Powell.—At the last meeting of the Association, Prof. Lloyd gave a *theoretical* investigation of certain results obtained by Sir D. Brewster relative to thin films from which polarized light is reflected. Besides completely explaining those results, Prof. L. infers that such films ought to give the portions of light reflected at their two surfaces differing in phase, and that the light should be consequently in general, *elliptically* polarized. The author of the present paper, before he was aware of the investigation of Prof. Lloyd, had made many observations on the elliptical polarization of light by reflection from metallic and other surfaces,—the method of observation being by the well known dislocation of the polarized rings. Some of these experiments went merely to prove the existence of elliptic polarization in cases where it had not previously been detected, as in certain minerals and other bodies in which it is seen, though of small amount. In other cases, the reflecting surface consisted of the thin films formed on polished metal by tarnish, by heat, or by the galvanic process of Nobili. In these instances, a verification of Prof. L.'s theory was afforded by *direct observation*. But, further,—these films give periodic colors, and in passing from one tint to another, the ellipticity, as disclosed by the form of the rings, underwent regular changes, passing from a dislocation in one direction to the opposite, through points of no dislocation or of plane polarization,

the rings being alternately dark and bright centered. This afforded a further field for the application of theory, and Mr. Airy investigated a formula for the rings under these varying conditions, with which the phenomena are in perfect accordance.

Mr. Nasmyth presented a paper on the *application of the law of Definite Proportions to the Stratification of clouds*,—prefacing it with the remark, that he was first led to speculate on this subject, by observing the arrangement of clouds in fine weather; when, towards the horizon particularly, they may be seen extended in parallel bands or stripes. He conceived that the excess of vapor floating in the atmosphere beyond what the air could combine with, formed clouds; and that the air in each electrical state, was capable of sustaining a definite proportion of vapor, and consequently that the clouds of one class or description floated, (in what might be called a plane of equal electricity,) at a uniform distance from the earth.

Sir William Hamilton made a brief communication *on a mode of expressing fluctuating or arbitrary functions by mathematical formulæ*. The subject was illustrated by diagrams, and excited great attention among the eminent men present. Prof. Jacobi said that Lagrange stated it as his opinion that it was not possible to express these functions by any mathematical formulæ. It appeared however to him, that Sir W. Hamilton had shown that it was possible.

The following communications were also made to the section.

On recurring Decimal Fractions, and a new species of Logarithms; by Mr. Anthony Peacock.

Observations on Oceanic Waves; by Mr. W. Walker.

General Considerations on the Analytic Theory of Equilibrium and Movement; by Prof. Braschman, of Moscow.

Report of the Commissioners for the restoration of lost standards of Weights and Measures, and upon their proposal for the introduction of a Decimal System; by the Dean of Ely.

On the meteorology of the Northern Atlantic and on the Southwest Monsoon of India; by Mr. Hopkins.

On improved Permanent Magnets, and the modes of determining their powers, with certain undescribed Phenomena in permanent magnets; by Rev. Dr. Scoresby.

On the Meteorology of the Province of Coorg in the Western Gâts of India; by Col. Sykes.

On the Magnetic Action of instantaneous currents of Electricity; by Prof. Marianini.

On the causes of the dissimilarity in the Frictional and Voltaic Electricities, with remarks on the Decomposition of Water by the former, and on Magnetism; by Mr. Goodman.

(*To be continued.*)

Art. XVIII.—*On the Rock Salt and Salines of the Holston;* by C. B. Hayden.

The salines of the Holston River, Virginia, are in the shales belonging to the lower portion of the *carboniferous* series. These salines therefore occupy a medial geological position in relation to the other salines of the United States; those of Kenawha, and the west generally, being higher up in the carboniferous rocks, and those of New York, according to Messrs. Conrad and Vanuxem,* belong to the Wenlock limestone group of the Silurian System. With the exception of those near Durham, in England, which are in the carboniferous rocks, they are geologically lower than those of Europe. The Holston salines† are situated in a valley, the bounding hills of which here assume an amphitheatrical arrangement, rendering the topography highly favorable by its drainage to the formation of those subterraneous reservoirs of water, which by solution of the saline matter of the adjacent strata furnish the wells with brine, and to this local topography they are indebted for their copious, constant, and uniform supply. This amphitheatrical valley, to which the discovery of brine has been confined, is about one fourth of a mile in breadth, and a mile in length. The centre of this valley consists of an earthy alluvium with a few interspersed pebbles and small bowlders, forming a flat, through which the salt wells are sunk. This alluvium is generally from eighteen to twenty feet in thickness, and reposes immediately upon the gypseous rocks, which are here in an advanced state of decomposition, consisting generally of blue and red clay, derived respectively from the blue and red shales, with which the gypsum is associated. Undecomposed rocks are rarely met with *in situ* in the wells and borings. The existence of salt springs and licks induced a successful search for salt water very early in the history of the country, but it is understood that the wells were shallow, and the brine both weak and limited in amount, although then sufficient to meet the demand.

The wells of the present establishments are six in number, but two of which are now in operation. They vary in depth

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* Vide New York State Geological Report for 1840.

† Saltville, Washington County, Va.

from two hundred to three hundred and eighty six feet. All the wells at the present site are within an area of three hundred feet; to this area, and the original wells one mile northeast of the former, the discovery of salt water has been confined. The same general section is presented in all the wells and borings, the upper eighteen or twenty feet consisting of the before mentioned alluvium, to which succeed alternating strata of red and blue clay and gypsum, the latter generally predominating, and which continue to the depth at which water is found, usually about two hundred feet. Throughout the valley already described, gypsum is found, generally at the depth of from eighteen to twenty feet. Further east the gypsum crops out, being underlayed with red and blue shales; immediately overlying the gypsum is a fetid crystalline limestone of a dark color, quite cellular, the cells being filled with crystals of carbonate of lime. At times the gypsum is separated from this rock by a thin seam of black pyritous slate. The gypsum is laminated, the plane of lamination having a general parallelism with that of its dip, which corresponds with that of the associated rocks, which is here highly inclined, showing that the deposition of the gypsum was anterior to the present disturbed condition of the rocks. The exposed portion of the gypsum is from twelve to twenty feet in thickness. The gypsum, unlike that of New York, is a nearly pure sulphate of lime; sometimes crystalline, sometimes granular, usually white, except when exposed to infiltration from the shales, when it has a slight ferruginous tinge, which is usually the case in the upper portion, where it is also more distinctly crystalline. Occasionally, it contains thin seams of a dark smoky color, and sometimes radiating crystals of the same hue. Running through the mass are frequently seams of beautiful fibrous gypsum. Well defined plates of *selenite* are rare. Gypsum of this general character occurs over a region of country of about half a mile in breadth and fifteen miles in length; it is generally worked to the depth of from twelve to twenty five feet, without being passed through; at several localities it has been ascertained to extend to the depth of two hundred and three hundred feet, and in one instance over four hundred feet.

In 1840, in sinking a shaft at Saltville, after passing through the usual thickness of alluvium, and alternating strata of red and blue clay and gypsum, nearly forty feet of which was solid gyp-

sum, *salt rock** was met with at the depth of two hundred and twenty feet, and continued to the bottom of the shaft, two hundred and seventy three feet, and was ascertained by boring to extend to the depth of three hundred and eighty six feet, without being passed through. In this well no water was met with. Nothing is known respecting the shape or extent of this deposit, except what is learned from the above mentioned shaft,—that it is there over one hundred and sixty six feet in thickness; it is however quite local in some directions, as it is not found in the neighboring shafts. If it is however, as seems most probable, the source of the brine, which has been worked for so many years undiminished in strength and quantity, it must be quite extensive. Much interest is imparted to this salt by its being the only fossil salt yet discovered in the United States. The salt is irregularly intermixed with red and blue clay and small fragments of shale, the salt usually predominating; occasionally the former are absent. These impurities, mechanically intermixed with the salt, were much more abundant in the upper portion of the bed, which was almost entirely free from them at the bottom of the shaft. The salt is compact, semi-crystalline, generally of a deep ferruginous color, though occasionally of a delicate flesh tint, and more rarely it is entirely free from coloring matter.

The planes of the shales intermixed with the salt have not that parallelism with each other which gravity would impart to them if deposited under quiet circumstances; and their irregular distribution through the salt, making with each other every conceivable angle, indicates agitation at the period of their deposition. Gypsum is occasionally interlaminated with the salt, and sometimes occurs in it, in fibrous crystals, more or less impregnated with salt. The salt is *anhydrous*, and nearly a pure chloride of sodium, as the following analysis will show.

Peroxide of iron, ...	0.470 gr.
Sulphate of lime, ...	0.446
Chloride of calcium, . . .	a trace.
" sodium, . . .	99.084

The local occurrence of a fault, the generally disturbed condition of the rocks, and the anhydrous nature of the salt, all argue the action of heat contemporaneously with, or subsequently to

* See the notice of this fact, published in this Journal, 1841, Vol. XLI, p. 214.

the deposition of the salt. Not having a freshly quarried specimen, I have been unable to ascertain whether the gypsum overlying the salt, or that associated with it, is anhydrous. This brine is saturated, and like that of Droitwich and Cheshire, is undoubtedly derived from the solution of fossil salt. This origin of the brine will explain its general uniformity in strength and composition in the different wells, when unaffected by infiltrated fresh water, as well as its slight variation in strength by continued working; it being well known that where the salt exists in the rocks interstitially, the brine is soon weakened by the exhaustion of the saline matter, rendering it necessary to deepen the shafts from time to time. Does not this origin explain the singularly low temperature of the wells? which in one of them is the same as that of the springs of the vicinity, and in the others, but one or two degrees above them. The temperatures of the respective wells are as follows—

Anthony well, 226 feet in depth, . .	54½°
Kings " 206 " " . .	53½
New " 214 " " . .	55½
Mrs. Preston's spring, (average temperature of the springs of the county,)	53½
Spring at salt works, ...	52½

This slight elevation of the temperature of the wells is less than is due to their depth, and may perhaps be owing to the cooling influence of saline solution. The brine differs in composition from the rock salt only in the absence of the peroxide of iron, and in containing a larger proportion of sulphate of lime; the former existing in the rock salt in the state of an insoluble peroxide is not dissolved, and the latter is derived from the associated strata. The annexed analysis is of the brine of the *new Artesian well*, bored a few months since to the depth of two hundred and fourteen feet, water having been obtained at the depth of one hundred and ninety three feet. One thousand grains of the brine contain—

Chloride of sodium, ...	240.52
" calcium, . . .	.08
Sulphate of lime, ...	5.35
Water, ...	754.05
Alumina, ...	a trace.

This brine therefore contains about twenty five per cent. of olid matter, twenty four per cent. of which is pure chloride of sodium. The richest of the New York brines, according to Dr. Beck, contain but about fifteen per cent. of this ingredient, which is there associated with the magnesian salts in addition to the ingredients contained in the Holston brine. A wine pint of this brine yielded of saline matter 2432.25 grains, equal in a gallon to 19458 grains, or 2.77 avoirdupoise pounds; therefore, by the ordinary process of direct evaporation, hereafter to be described, eighteen gallons of the brine will produce one bushel of salt, weighing fifty pounds. As the object of the above experiment was to determine the *practical* value of the brine, the salt was not reduced to absolute dryness, but to the ordinary dryness of the salt of commerce.

The annexed table from Van Rensselaer, with Dr. Beck's additions, to which I have added the result of my experiment upon the Holston brine, will afford a comparison of the strength of the different brines of the United States.

At Nantucket, Mass.	350	gals.	sea-water	yield	1	bushel	of salt.
Boon's Lick, Mo.	450	gals.	brine	yield	1	bushel	of salt.
Conemaugh, Pa.	300	"	"	"	"	"	"
Shawneetown, Ill.	280	"	"	"	"	"	"
Jackson, Ohio,	213	"	"	"	"	"	"
Lockhart's, Miss.	180	"	"	"	"	"	"
Shawneetown, Ill. 2d saline,	123	"	"	"	"	"	"
St. Catherine's, U. C.	120	"	"	"	"	"	"
Zanesville, Ohio,	95	"	"	"	"	"	"
Kenawha, Va.	75	"	"	"	"	"	"
Grand River, Ark.	80	"	"	"	"	"	"
Illinois River, Ark.	80	"	"	"	"	"	"
Muskingum, Ohio,	50	"	"	"	"	"	"
Onondaga, N. Y.	41 to 45	"	"	"	"	"	"
Holston, Va.	18	"	"	"	"	"	"

The entire absence from this brine of the magnesian salts, so prejudicial in other brines, and the extremely minute proportion of the other salts present, precludes the necessity of those preliminary operations practiced at other works for the separation of these impurities. The water is first pumped into large reservoirs, where the impurities held in mechanical suspension are

deposited; it is thence conveyed to the kettles, and evaporated by rapid ebullition. As the salt is deposited, it is removed in wicker baskets, and suspended over the kettles until it is dry. The cast iron kettles are segments of spheres, of about the capacity of forty gallons; these are arranged in pairs, in parallel rows, over an oblong furnace heated by wood, the heat being applied directly to the bottom of the kettles. In the arrangement of the kettles and the construction of the furnace, little regard is paid to the economy of fuel, or heat. By direct evaporation, at a high temperature, salt of a superior character is expeditiously made; but the process is attended with the serious inconvenience of the formation of a hard saline incrustation on the bottom of the kettles, owing to the intense heat to which the salt, as it is deposited, is subjected. This incrustation, called by the workmen "*blocking*," is sometimes fused into a vitreous mass, and is generally crystalline, adhering with such tenacity to the sides of the kettle, as to require the pickaxe for its removal. A serious increase of expense attends the delay necessary for the removal of this incrustation, as well as from the more rapid corrosion of the iron kettles. That this incrustation is not, as is generally supposed, a deposit of the more insoluble impurities of the brine, will be seen from the following analysis. Two specimens, selected by the proprietors as representing the average character of the "blocking," proved nearly pure salt. They were of a snowy whiteness, compact, hard, laminated, highly crystalline and anhydrous.

	No. 1.	No. 2.
Sulphate of lime,	4.892 grs.	9.314 grs.
Chloride of calcium,	.068 "	.050 "
" of sodium,	95.040 "	90.510 "
Carbonate of lime,*	a trace.	.126 "

The annoyance occasioned by the "blocking," would be entirely obviated by evaporation at a lower temperature, which would also improve the character of the salt, as the slower the evaporation, the more perfect the crystallization. At Chesh-

* The occurrence of carbonate of lime in the "blocking," is probably attributable to its existence in the brine in a proportion so minute as to escape detection in the quantity operated upon; or possibly it may be held in mechanical suspension in the brine, that operated upon having been permitted to stand until all the mechanical impurities were deposited and the clean water decanted off.

ire, where the brine is similar to this, the water is evaporated at a temperature below ebullition, in shallow wrought iron pans of an area of from eight to twenty square feet, which last for several years. A pan of this description, brought over by the Hon. William C. Preston, was burnt out in a few months at Saltville. The requisite low and uniform temperature could be readily attained by the use either of steam or hot water, as a heating agent, applied in the manner so frequently done in the manufacture of sugar and similar departments of the arts. This modification in the manufacture, besides avoiding the "blocking" and improving the salt, would economize fuel, and the latter consideration should urge itself with serious force upon the proprietors, occupying as they do a region where forests must soon be exhausted, and where geology forbids the hope of the discovery of coal.

A large proportion of the salt manufactured at Saltville, is made by the process of direct evaporation, already described. A whiter and purer salt is made by first concentrating the brine in a wooden cistern, through which steam is conducted in a cast iron pipe, which sufficiently concentrates the brine to produce a deposition of most of the sulphate of lime; the concentrated brine is then conveyed to the kettles, and evaporated as in the other process. The salt thus manufactured is purer, more distinctly crystalline, and has a white satin lustre. The following analysis of the two varieties of salt, will show the difference in their composition. No. 1, is the salt made by the process last described; No. 2, by the first process, of rapid evaporation.

	No. 1.	No. 2.
Sulphate of lime,	1.444	1.820
Chloride of calcium,	.016	.034
" of sodium,	98.540	98.146

The two establishments at Saltville manufacture annually two hundred thousand bushels of salt.

Abingdon, Va., Nov. 30th, 1842.

ART. XIX.—*Description of a Carbon Voltaic Battery;* by B. SILLIMAN, Jr., A. M., of the departments of Chemistry, Mineralogy, and Geology, in Yale College.

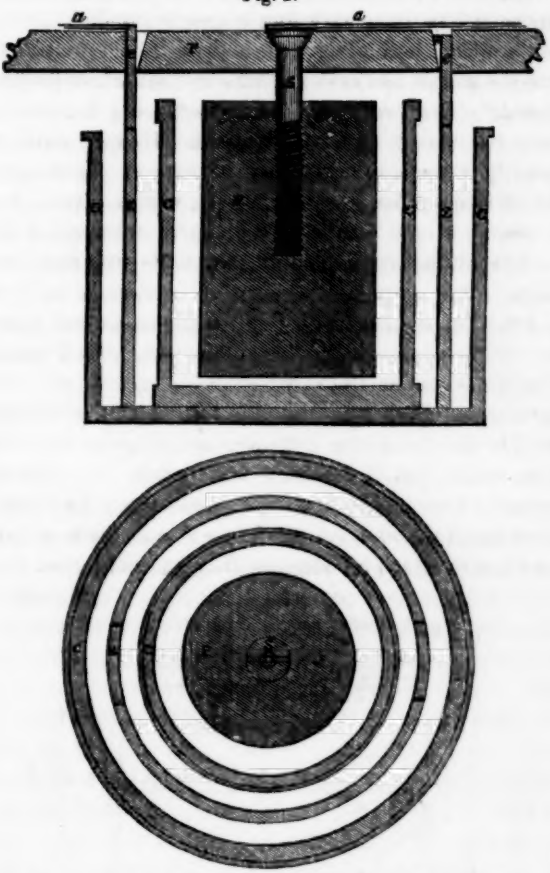
IT was mentioned in the last number of this Journal (Vol. XLIII, p. 393,) that a battery of considerable series was constructing by me on a plan, in many respects, similar to the nitric acid battery of Mr. Grove, but differing in the important particular of employing carbon as the negative element, in place of the platina of Mr. Grove's arrangement. I have since completed this battery, and made a series of experiments with it, and propose now briefly to describe it.

The substitution of plumbago for the platina, and the use of circular in place of flat cups, with certain details of less importance, are the principal points of difference between this and other nitric acid batteries. Fig. 1, gives a vertical and cross section of one member of the battery. The lettering is the same in both, and the drawings are on the scale of half the real size. C, the external cell, is of a peculiar glazed ware, which withstands the action of acids remarkably well.* This cell contains the dilute sulphuric acid, and is four inches in diameter, and three in height. Z is the zinc element, which is of cast zinc, and amalgamated with mercury. It is cast with two ears, (*e e*, in vertical section,) which pass up through the bar of the frame F, in which they are supported, being crowded by the small wedges *w*. The zinc rests on the bottom of the outer cell C. C, the inner cell, is of porous queen's ware, strengthened by a band of the same material at top and bottom, and resting within the zinc on the bottom of the outer cell; it is five eighths of an inch less in diameter than the zinc. In this cell is placed the nitric acid. P is the plumbago, which is in the form of a solid cylinder, three inches high, and two in diameter. It is supported by the brass screw S, which passes through the frame F. By this arrangement, one objectionable feature of Mr. Grove's battery is avoided, viz. the elements can be raised from the acids when not in use, each member dripping its acid into its own cup, and the whole being securely fixed

* This ware, as well as the porous cups, is manufactured at Jersey City pottery, where any similar articles can be obtained to order.

in the frame F F, figure 2, can rest on the edges of the outer cups until the experimenter is ready to continue his observations.

Fig. 1.



In the usual form of Grove's battery, the porous cell is inextricably confined within the zinc plate, which rises on both sides of it, and confines it in one position; so that there is no way to charge the cells, except by carefully pouring through a funnel, and they must be discharged when done with, by inclining the apparatus gradually on one side, until the nitric acid runs out. In the form here proposed, the cups are all filled very expeditiously, by pouring into each a measured quantity of acid; and during

this operation, the inner cells may be removed from the outer, and allowed to stand in some convenient vessel until wanted for use, so that the intermixing of the acids, which always happens, more or less, in twenty four hours, is avoided.

The plumbago here employed, is the artificial mixture made at the crucible works, and crowded into moulds of the proper shape, and baked.* Only in a few instances have I found a cylinder fail from the action of the nitric acid. Having failed to procure readily a sufficient quantity of natural plumbago, I was induced to employ the present material, which, although inferior in the energy of the results to the native mineral, is still very good. The difficulty in procuring suitable cylinders of native plumbago, arises not from the want of an abundance of the material, but from the occurrence of numerous natural joints, even in that which seems externally quite sound, which cause them to fall to pieces under the saw.

The mode of connecting the plumbago with the zinc, suggested itself to me from the difficulty and expense of employing mercury, and I find it in every way better. The brass screw enters nearly two inches into the plumbago. Its neck passes snugly through the wood of the frame F, and the head presents a large surface to attach a connector from the next zinc.

Fig. 2.

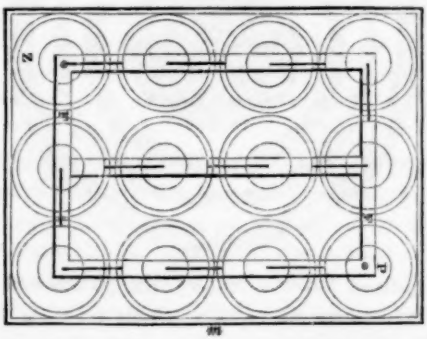


Fig. 2 shows the mode of arrangement adopted for each group of twelve members; the frame F F supports both the zincs and plumbagos, and by it they are removed from the cups; a connector of copper passes from the screw of each plumbago to the

* To be obtained of J. W. Ingall, Taunton, Mass.

zinc of the next pair, where it is soldered to the ear *e*, (fig. 1.) The other ear of the same zinc is cut off below the surface of the bar, and serves only to support the zinc firmly in its place. This arrangement, it will be seen by inspecting the figure, brings out the poles at the diagonally opposite corners, where a gallows screw is fixed for attaching the series to the next group of twelve. The twelve cups are confined in one position, by the mahogany tray *m*, in which they are easily moved from place to place.

In the present battery there are eight groups of twelve pairs each, or ninety-six pairs in all, exposing about one thousand two hundred square inches of plumbago to three thousand of zinc. The series may be connected with each other consecutively, and used with the intensity of ninety-six pairs, and the quantity of one member; or they may, by the ingenious suggestion of Dr. Hare in similar cases, be made to work with the intensity of twelve pairs, and the accumulated quantity of eight groups, each of twelve series. This beautiful mode of modifying the power and action of a battery of a given number of plates, has been described by Dr. Hare, in the Proceedings of the American Philosophical Society for 1840.*

The effects of this battery have given me much satisfaction. In some respects I feel a good degree of assurance that it is superior to any other form of voltaic battery with which I am acquainted. I regret that it is not in my power to compare it more accurately with other batteries, and especially with Mr. Grove's, as I have not now at my disposal a series on his construction.

It is not supposed that the plumbago is quite equal to platina for the negative element. Yet its surface is well calculated for an electrode, and its conducting power is remarkable, when we remember that it is not a metal, and indeed it seems to be but little behind platina in this respect, especially as, from the size of the mass employed, it may offer less resistance to the passage of an electric current, than may be the case with a thin strip of platina.

This is well worthy the name of a "constant battery," as its action is continued for many hours, with apparently little decline. The longest period in which I have employed one was eight

* The same account may be found in Vol. xl, p. 49, of this Journal.

hours, and at the end of that time I could perceive no sensible diminution of energy.

The ninety-six pairs, after having been in action some time, yielded from the decomposition of water, four cubic inches of mixed gases in twenty-seven seconds, or about nine cubic inches per minute. A series of nine hundred pairs of Wollaston's arrangement in this laboratory, (being as is supposed the largest battery on this construction in existence, in which the whole is plunged by one movement,) each zinc plate four by ten inches, gave at the same time, when excited in the usual way, four cubic inches in fifteen seconds. The time was therefore as fifteen to twenty-seven; but the surface of metals opposed to each other in the larger series, is one hundred and fifty-six thousand square inches, and that in the constant battery is four thousand two hundred. I infer from this, that *cæteris paribus*, the new battery must vastly exceed the old form, in all the classes of effects produced, but am unable from the above data to draw any definite ratio.

The brilliancy of deflagration produced from this arrangement is very great, particularly in the case of a continuous stream of mercury flowing from the narrowed point of a bent funnel, into which is thrust one pole of the battery, and falling on the surface of a pair of the same metal connected with the other pole. The inspection of this splendid combustion for several minutes with unguarded eyes, resulted in a severe inflammation, which confined me for a short time to a dark room.

In the note referred to, in the last volume of this Journal, page 393, I stated that a battery of six members of plumbago one inch in diameter, and two inches high, opposed by an amalgamated zinc coil two and a quarter inches in diameter, and two inches high, (equal to thirteen and a half square inches of zinc, and six inches of plumbago, or about one hundred and twenty square inches of opposing surfaces in the whole battery,) gave the remarkable result of four* cubic inches of mixed gases from water, in a little less than fifty seconds, and maintained fourteen inches of number thirty platina wire coiled into a spiral, at full incandescence, for nearly an hour! The following is taken from Mr. Grove's original article describing his battery, shortly after its invention. Speaking of his former account of his battery, (before the British Association at Birmingham,) he says, "I underrated

* Erroneously printed *in loc. cit.* 5.

very considerably the power of the battery, which with proper arrangements liberates six cubic inches of gas per minute, heats to a bright red seven inches of platinum wire one fortieth of an inch in diameter, burns with beautiful scintillations steel needles of a similar diameter, and affects proportionately the magnet. The battery consists of four pairs of zinc and platinum foil plates, each metal exposing a surface of fourteen square inches; the whole occupies less space than a cube four inches in the side."

I was agreeably surprised on reperusing this article, after the publication of my note above referred to, to find how favorable the comparison was between the plumbago and the platina. The platina battery of Mr. Grove exposed one hundred and twelve square inches of surface, the plumbago one hundred and twenty; the former gave one cubic inch of gas in ten seconds, the latter one cubic inch in twelve seconds.

The form of carbon most efficient in voltaic circuits, is that exceedingly hard and pure carbon which is deposited from coal gas on the heated inner surface of the retorts of the gas works; but it is so difficult to work it into shape, and to attach it to other substances, that it cannot be availed of. I have tried anthracite and bituminous coal, but not with success.

In the battery described in this article, as actually constructed, the two elements were, by mistake, placed at a greater distance than I had intended, and no doubt much power is by that means lost. In another apparatus which I am constructing, it is my intention to remedy this defect. Nearness of approach between the elements cannot be too much looked to in such arrangements.

In the 'Archives de l'Electricité,' (No. 1, p. 262,) M. De La Rive gives an account of some experiments made by him with a Grove's battery of forty pairs. He states the luminous arch produced between the poles, after contact and gradual separation, to be *one inch* in length. That produced by the subject of this paper, was about two inches, but was not accurately measured, nor was it taken in vacuo.

By way of illustration, I would remark, that the large Wollastonian arrangement before alluded to, when new, gave an arch in the open air of seven inches, and may now be usually depended on for an arch of from four to five inches in length.

M. De La Rive further observes that the transfer of particles of carbon from the positive to the negative pole is evident, and

then goes on to describe the phenomenon. Dr. Kane also and Prof. Daniel, in their several works, refer to this fact as of recent observation. Now it is not a little remarkable, that at this late day this fact should be esteemed either rare or wonderful, since the same phenomena were more than twenty years ago described by Dr. Hare and Prof. Silliman, and have been witnessed ever since by all who have used or seen the powerful deflagrators of Dr. Hare.* Is not the carbon, when thus transferred, in the state of vapor?

It is not my wish in this article, to extol the merits of my battery, at the expense of any other, but merely to call attention to what I believe to be an important modification of Mr. Grove's admirable contrivance.

Yale College Laboratory, Dec. 14, 1842.

P. S. Since this article was written, I have, by simple accident, found (while looking for another matter) in the London, Edinburgh and Dublin Philosophical Magazine for January, 1840, page 35, a letter to the editors, from Mr. J. T. Cooper, giving notice of his having employed plumbago and other forms of carbon in voltaic combinations, with highly satisfactory results. This article I had not seen, and it proves again what has been so often proved before, that the same end will often suggest the same means; but undoubtedly the claim of priority in this case belongs to Mr. Cooper, to whom I cheerfully yield it. I have not, however, heard that any one has taken advantage of Mr. Cooper's suggestions to construct a large battery on this plan.

Dr. Hare also informs me that a similar plan was original with himself, and that a carbon battery was contrived in Germany more than a year since.

Prof. S. F. B. Morse, (to whom I cheerfully acknowledge my obligations,) has contrived a battery for moving his electro-magnetic telegraph, which surpasses in constancy any heretofore made. He assures me that he has had it in good working condition without interruption seven or eight days. Its peculiarity is in interposing between the zinc and platina of Mr. Grove's arrangement a second porous cell, containing strong sulphuric acid, which interrupts the mixing of the nitric with the dilute sulphuric acid, or *vice versa*.

* See a correspondence on this subject in the 5th vol. of this Journal, (1822,) p. 108 *et seq.*, between Prof. Silliman and Dr. Hare.

Art. XX.—*Bibliographical Notices.*

1. *Report on the Geology of Connecticut;* by James G. Percival. Published under the direction of the Commissioners appointed by the Legislature. New Haven, Osborn & Baldwin, 1842. pp. 495, 8vo., with a map.—This long expected report has just come into our hands wet from the press, as we are closing our present number. From the cursory examination we have been able to give it, we are strongly impressed by the great amount of condensed information it contains, relative to the exact limits and extent of our several geological systems—the primary, the secondary, and the intrusive or plutonic rocks. When we remember the learning and talent of the author, and the laborious accuracy with which he investigates every subject which occupies his attention—that he began this exploration in 1835, and has pursued it with great diligence until the present year—that he has been personally and on foot in contact with every one of the four thousand six hundred square miles in the state; and has carefully collated and compared more than eight thousand specimens, beside much more numerous dips and bearings—it cannot be doubted that he has laid the foundation for all future investigations into the geology of Connecticut.

Dr. Percival tells us, that "this report is but a hasty outline;" by this we presume he intends that the full exposition of his theoretical opinions is not given; but we feel assured as to the fullness and accuracy of the facts, and that it will prove a satisfactory solution of a problem of the highest practical as well as scientific importance—the exact determination of the geological system of the state.*

That this report will meet the expectations of the unscientific but otherwise intelligent bulk of the reading population of the state, would be unreasonable to expect; it is not the object of the book to please or amuse, nor even to point out new sources of wealth, (though this duty has not been neglected whenever opportunity offered,) and we are not to suppose, that the greater portion of the legislative assemblies who have voted grants of money for its completion, looked mainly to these secondary ends. But we have the satisfaction of knowing that we have for all future time a faithful guide to our explorations, and a standard of comparison at once full and simple. The scientific geologist, who reads Dr. Percival's report, will only regret that he has not the advantage of the full exposition of the author's theoretical views on many points of interest to science at large—views which we know the accomplished au-

* We are however not a little surprised, that he hesitates in expressing an opinion as to the age of our secondary. The facts published by others, render the determination almost beyond dispute, that our sandstones belong to the *new red* group.

thor possesses, and which, we trust, with the aid of the abundant unused notes and materials at his disposal, he will at some future time make public for the benefit of his fellow laborers.

It will ever be gratefully remembered, that we owe the present report on our geology, as well as that of Prof. Shepard on the *mineralogy* of the state, (vide this Journal, Vol. xxxiii, p. 151,) to the enlightened zeal of the Hon. Henry W. Edwards, under whose advice, when in the gubernatorial chair in 1835, the measure was originally proposed, and who has since uniformly sustained it by his official and personal influence.

2. *Natural History of New York. By Authority. Part first—Zoology.* By James E. De Kay. New York, D. Appleton & Co., and Wiley & Putnam, 1842. 4to. introduction, pp. 188; text, (zoology, class mammalia,) 146; 33 plates.—The appearance of the first volume of the final report of the commission of scientific men appointed to survey the natural history of the important territory of New York, is hailed by all lovers of science in America as no ordinary occurrence. This is the first of ten volumes, which are to be divided as follows: General Introduction; Part I, Zoology, by James E. De Kay; Part II, Botany, by John Torrey; Part III, Mineralogy, by Lewis C. Beck; Parts IV and V, Geology and Palæontology, by William W. Mather, Ebenezer Emmons, Lardner Vanuxem, and James Hall.

It is not our present purpose to enlarge on this subject. The volume before us is a fair earnest of our expectations; mechanically the best done of any report yet published by our several states, it will doubtless prove equal to the demands of public expectation in its detail. The plates are better than those before published in any state report in this country; in general, they are very good, particularly the copper or steel plates, while the lithographs are not a fair example of the present state of that art.

Governor Seward's introduction is made up of matters and things in general, touching the history and progress of the state of which he has been for four years past the political head. It is interesting enough doubtless to the inhabitants of the "*Empire State*," and contains a succinct account of the origin and progress of the survey of which the ten volumes in question are the results; but it would be more appropriate for a gazetteer than as the introduction to the *Natural History* of a territory of forty six thousand two hundred square miles and lying in the temperate zone.

When the publication is completed, which we understand will be as soon as practicable, it is our intention to give in considerable detail, a *coup d'œil* view of the general scope of this great work, which, accomplished by so many laborers in seven years of research, and at a cost of one hundred and thirty thousand dollars, will ever redound to the liberality and enlightened zeal of the noble commonwealth whose work it is, and to the fame of the numerous gentlemen of science by whose united efforts it has been accomplished.

3. *Lectures on the application of Chemistry and Geology to Agriculture. Part II, on the Inorganic Elements of Plants.* By J. F. W. Johnston, M. A., F. R. S., &c. &c. New York, Wiley & Putnam, 1843. (American edition,) 12mo. pp. 175.—Just one year since, we gave (Vol. xlii, p. 187) our favorable opinion of the first part of Prof. Johnston's Lectures. It is therefore unnecessary to repeat those remarks here, which are only confirmed by the character of the present portion of the work.

According to the present state of our knowledge on this subject, the inorganic constitution of the soil and of plants, is a most essential and important branch of scientific agriculture. It is evident that no plant contains any inorganic element not contained in the soil in which it grows; it is certain that these inorganic constituents, although present in small proportion, in some way not very well understood, are essential to the growth and perfection of the plants containing them. An accurate knowledge, therefore, of the composition of the ashes of all agricultural plants is highly important, in order to determine if a given soil contains all that is essential to the growth of any particular species or family. Hence the importance of correct analyses of soils, in order to a judicious and effective application of manures and alteratives. Wheat cannot grow and perfect its fruit, without phosphate of lime; the double phosphate of lime and magnesia is essential to potatoes, and hence bran, which is peculiarly rich in this salt, is a very valuable manure for the potato. One of the most remarkable features of recent scientific progress, is the wonderful amount of talent and research which has within three or four years past been so successfully applied to the improvement of agriculture. A short time since, and no agricultural chemistry could be found, save the original treatises of Davy and Chaptal, and agricultural geology was an unknown term. Now, the shelves of the bookseller are crowded with new publications of this character, and every news-letter and advertiser contains some new announcement of agricultural works, while the strangest and most desirable part of the phenomenon is, that the demand is equal to the supply.

We are passing through a new era in this department of science, and when the present excitement has passed away, and we look for the actual progress made, the new principles established, and improved methods adopted, we venture to predict that in the changes effected, Prof. Johnston's works will be found to have acted an important part.

4. Elements of Chemistry, *including the actual state and prevalent doctrines of the science;* by the late Edward Turner, M. D., F. L. S. L. & E. Seventh edition. Edited by Justus Liebig, Ph. D., M. D., &c., and William Gregory, M. D., &c. London, Taylor & Walton, 1842. pp. 1274, 8vo.—At length after an interval of five years, is this long expected edition brought to a close by the publication (about a month since)

of the "second supplement," containing about 160 pages. The delay in its appearance, has resulted from a desire on the part of the editors to include in it all the recent discoveries of the science, and especially the new views of Dr. Liebig, contained in his Animal Chemistry, published last June. Those who purchased the first part of the sixth edition of this work, and have waited till now for its completion, will have the pleasure of repurchasing the whole, if they mean to be possessed of the entire seventh edition.

The chapters on salts have been remodelled in this edition, and the present views respecting salt radicals explained; while the entire organic chemistry has been rewritten, and many laborious series of researches undertaken at Giessen in reference to this end. A condensed summary is given of Prof. Liebig's views of the changes which occur during the life, growth, and nutrition of vegetables and animals; and a few pages added (as part fourth) on chemical analysis, taken chiefly from the recent work on that subject by Mr. Parnell.

5. *Elements of Chemical Analysis, Inorganic and Organic;* by Edward Andrew Parnell, Chemical Assistant in University College, London. London, Taylor & Walton. 1842. pp. 309, 8vo.—An original work on this subject in English and written in England, is indeed a rarity which cannot be allowed to pass unnoticed. It is not to be expected that in a work of 300 pages there should be any thing like the fullness of Rose's elaborate manual in two volumes. But this work was evidently written for the uninitiated in the analytic art, and in that view it is an excellent book. Indeed Mr. Parnell's book supplies a deficiency which has heretofore existed in the chemist's library, of a work intended for the non-proficient. The knowledge assumed to be familiar to the reader in the admirable treatise of Heinrich Rose is quite beyond the acquirements of most students, and hence his long, full descriptions of unknown processes serve to only darken counsel by words without knowledge.

A pretty full and well arranged set of tables, evincing much labor and care, follows the introduction, and contains in a convenient form for reference the most usual and important behavior of the various oxides and acids with reagents.

The various manipulations and necessary apparatus on which the success of analytical operations depends, are the subject of the first part of the book. Numerous and well drawn wood-cuts illustrate the text whenever such illustration is important. The quantitative analysis of various bodies is ably discussed, and the difficult analysis of natural silicates illustrated by well selected examples. This part of the work contains tables of great value and convenience to the student. The use of the blowpipe, and the treatment of substances in the inner and outer flame, is fully treated, which, with a description of the modes of detecting poisonous substances, (arsenic, lead, mercury, &c.) finishes the first part of the work.

Qualitative analysis forms the second part, and is mostly a condensation of the rules of M. Rose. A condensed view of the outline of organic analysis is then given, and the work concluded by a set of tables for calculation of the results of an analysis. These tables, so important to the working chemist, and which form so valuable a portion of Mr. Rose's work, were strangely omitted by Mr. Griffin in his English translation of that work.

6. *The Chemical Gazette, or Journal of Practical Chemistry, in all its applications to pharmacy, arts, and manufactures;* conducted by William Francis and Henry Croft, late students in the universities of Berlin and Giessen. London, Nov. 1, 1842. No. 1, (published on the 1st and 15th of every month,) pp. 32, 8vo.—Chemistry is certainly on the ascendant in England. The establishment and activity of the Chemical Society, the number and value of the recent chemical treatises—witness Daniel, Kane, Graham, Turner, (by Gregory and Liebig,) Parnell, Brande, &c., and now the issuing under such favorable auspices of this Journal—all show conclusively that both the demand and the supply is better than heretofore. In all this can be seen the influence of the British Association, and the benefit (as one part of the results of that institution) of a more familiar and personal knowledge of continental chemists and their doings.

Messrs. Francis and Croft have for some time past contributed valuable notices of what was doing on the continent to the Philosophical Magazine. The "Gazette," they state, is intended to be eminently practical, giving to the working chemist and pharmaceutist, the earliest possible information of all that occurs on the continent and in England, which is important to be known by them. The complexion of this their first number bears out their proposal. We copy the table of contents.

Introduction: Table of Elementary Bodies, with their Symbols and Atomic Weights. Scientific and Medicinal Chemistry.—On testing for Arsenic in cases of Poisoning: On the conversion of Ligneous Fibre into Starch, by Prof. Liebig: On Digestion: On Glucinum and its compounds: Fluid of the *Spina Bifida:* Solubility of the Sesquioxide of Iron in Carbonate of Ammonia: Analysis of the Gall Stone of a Sheep: Action of Sulphuretted Hydrogen on an acid solution of Zinc and Arsenic Acid. Analytical Chemistry.—Separation of Chloride of Magnesium from the Chlorides of Potassium and Sodium: Separation of Zinc from Nickel and Cobalt: Separation of Lead from Bismuth: Tests for Iodine and for Copper. Pharmacology.—Adulteration of Saffron: On the Barks of commerce. Chemical Preparations.—Preparation of finely divided Calomel, of Hyperchloric Acid, of pure Sulphuric Acid, of Chloride of Zinc, of pure Potassa and Soda: *Unguentum contra Tineam:* Employment of Sulphuret of Iron in cutaneous

diseases. CHEMISTRY APPLIED TO ARTS AND MANUFACTURES.—Theory of Saponification, by Prof. Liebig: On the preparation of Ultramarine. REVIEWS.—Elements of Chemical Analysis, by E. A. Parnell, Esq. PATENTS FOR IMPROVEMENTS IN CHEMICAL PROCESSES.—Improvements in the manufacture of Sulphuric Acid; in the operation of Tanning.

7. *Elements of Chemistry, including the most recent discoveries and applications of the science to Medicine and Pharmacy, and to the Arts;* by ROBERT KANE, M. D., M. R. I. A., &c. &c. An American edition, by JOHN WILLIAM DRAPER, M. D., Prof. of Chem. in the University of New York. New York, Harper & Brothers, 1842. pp. 704, 8vo.—The appearance of an American reprint of Dr. Kane's excellent treatise was very acceptable to all devoted to the science in this country. It has been we understand extensively adopted as a text-book in many of our colleges; and from a careful perusal of it and use in our own laboratory for two years, feel assured that it is the best book now accessible to the American student. Dr. Draper has not added (nor was there occasion that he should add) much to the original text, but where he has done so it is generally from the results of his own researches, particularly in the physical part. We might mention as an instance occurring to us at this moment, his explanation of the theory of Daguerre's process. We regret that it was thought advisable in this edition to curtail the index. No book can have too full an index, and least of all a chemical book.

8. *An effort to refute the arguments advanced in favor of the existence of the Amphide Salts, or radicals, consisting, like cyanogen, of more than one element;* by ROBERT HARE, M. D., Professor of Chemistry in the University of Pennsylvania. 23 pages, pamphlet.—This is a very acute and able discussion of an obscure and difficult subject, to which we have neither space nor time to advert, in explanation of the title cited above. An attempt to subvert the present nomenclature of the oxy-salts, and to introduce a new arrangement of their elements, so as to make them correspond with the haloid salts, appears to us very unnecessary, and to be unsupported by any reasons, sufficiently important, to justify so annoying an innovation. Men of acute minds may arrange mentally the chemical atoms so as to produce results which harmonize, and leave no fractions to be disposed of. But bounds should be set to these intellectual recreations, especially when they produce a host of new names for principles whose existence cannot be proved, because they cannot be isolated. Many of the names, for example, recently introduced into the organic chemistry, are uncouth, complex, hypothetical, and at war with euphony. Dr. Hare's argument, as regards the new nomenclature of the oxy-salts, appears to us to be conclusive, and we trust that the beautiful language so long in use will not be set aside, nor the still more beautiful harmony of the saline elements disturbed.

9. Murchison's *Silurian System.*—It will be gratifying to geologists and naturalists generally, in this country, to learn that Mr. Murchison's celebrated work, on the Silurian System, can now be had at about half the price which it has heretofore been sold at in this country. We are authorized by Mr. Murchison to offer the work to *American* naturalists for £5 sterling. It may be had at this rate of Messrs. R. & J. E. Taylor, Red Lion Court, Fleet street, London, or more conveniently of Messrs. Wiley & Putnam, 161 Broadway, New York, and 35 Paternoster Row, London, for the same price, plus the duty and transit. This reduction is made expressly to promote the circulation of the work in this country, where the Silurian rocks hold so prominent a place, and is not extended to England, where the price is as originally, £8.

We may also state that Mr. Murchison will publish his researches in Russia next summer, in a separate and extended form.

10. *The Climate of the United States and its Endemic Influences, based chiefly on the records of the Medical Department and Adjutant General's office, United States Army;* by Samuel Forry, M. D. New York, J. & H. G. Langley, 1842. pp. 378, 8vo.—Dr. Forry has been long known by his meritorious labors in various departments of statistics, particularly by his "report on the sickness and mortality in the army of the United States," and the Army Meteorological Register. In the present work, advantage has been taken of the materials collected by the returns from the various military posts, and also from other sources, to draw out a general and comprehensive view of the climate of the United States, and of North America in general. We believe this task has never been before attempted, certainly not with any thing like the advantages possessed by Dr. Forry. The work seems well done, and the author deserves the commendation of all for his labors in this new and important field of investigation.

11. *Boston Journal of Natural History, containing papers and communications read before the Boston Society of Natural History.* Vol. IV, No. 2. Boston, C. C. Little & James Brown, 1842. pp. 128, 8vo.—The Journal of the Boston Society is now published with so much exactness as almost to acquire the character of a regular periodical work. In our 42d volume, p. 379, we gave the contents of the part of Vol. IV preceding the present part. The articles in this part are as follows:

Remarks upon Coral Formations in the Pacific, with suggestions as to the causes of their absence in the same parallels of latitude on the coast of South America, by Joseph P. Couthouy, continued from p. 105: Descriptions of some of the species of Naked Air-Breathing Mollusca inhabiting the United States, by Amos Binney: Additional descriptions of, and observations on, the Fishes of Massachusetts, by D. Humphreys Storer, M. D.: An inquiry into the distinctive characteristics of the Abo-

riginal Race of America, by Samuel George Morton, M. D.: Descriptions and figures of the Araneides of the United States, by Nicholas Marcellus Hentz: Descriptions of the Fishes of Lake Erie, the Ohio River, and their tributaries, by Jared P. Kirtland, M. D., continued from p. 26.: Description of a species of Helix, newly observed in the United States, by Amos Binney: Observations on the habits of the Python Natalensis, by Thomas S. Savage, M. D.: Observations on the characters and habits of the Ocellated Turkey, (Meleagris Ocellata, Cuv.,) by Samuel Cabot, M. D.: On the existence of (Siliceous?) Spiculæ in the exterior rays of Actinia, and memoranda concerning the Siliceous Animalcules of Boston, by J. W. Bailey: Enumeration of the Fishes of Brookhaven, Long Island, with remarks on the species observed, by Wm. O. Ayres.

The preponderance of zoological matter over the other branches of natural history, strikes the eye at once in glancing over this table of contents. This is of course in a great measure accidental, but it is nevertheless true, that the number of contributors in the zoological departments have been greater generally in this Journal than in botany, palæontology, and mineralogy. We observe in this number some typographical errors, an unusual thing in the Boston press.

12. *The North American Sylva; or a description of the Forest Trees of the United States, Canada, and Nova Scotia, not described in the work of F. Andrew Michaux, and containing all the Forest Trees discovered in the Rocky Mountains, the Territory of Oregon, down to the shores of the Pacific, and into the confines of California, as well as in various parts of the United States. Illustrated by 122 fine Plates.* By Thomas Nuttall, F. L. S., &c. &c. In 3 vols.—Vol. I, (first half.) Philadelphia, J. Dobson, 1842. pp. 55, royal 8vo.; 19 plates.

The first half volume of Mr. Nuttall's continuation of Michaux's Sylva made its appearance about three months since. We are happy to say that Mr. Nuttall's part of this justly esteemed work is issued in a mechanical style quite free from the faults of the reprint of the old portion, of which it forms the continuation. This part is on fine paper, well printed, and adorned with brilliantly colored plates. Mr. Nuttall's labors have probably nearly doubled the number of trees described by Michaux. This it seems is the farewell labor of Mr. Nuttall in the United States, as at the close of a beautiful preface to the present volume, in which he gives a very brief account of his wanderings, he says: "But the 'oft told tale' approaches to its close, and I must now bid a long adieu to the 'new world,' its sylvan scenes, its mountains, wilds, and plains, and henceforth, in the evening of my career, I return, almost an exile, to the land of my nativity!"*

* Mr. Nuttall left this country for England about one year since.

We append a list of the principal species mentioned in this half of the first volume, and when the remaining parts are before the public, will take more extended notice of it as a whole.

Oak.—Western oak, *Quercus garryana;* Holly-leaved oak, *Q. agrifolia;* Small-leaved oak, *Q. *dumosa;* Rocky Mountain oak, *Q. undulata;* Douglass oak, *Q. Douglassi;* Dense-flowered oak, *Q. densiflora;* Lea's oak, *Q. Leana.*

Chestnut.—Dwarf chestnut, *Castania alnifolia;* Golden-leaved chestnut, *C. chrysophylla.*

Birch.—Western birch, *Betula occidentalis;* Oval-leaved birch, *B. rhombifolia.*

Alder.—Oregon alder, *Alnus *Oregona;* Thin-leaved alder, *A. tenuifolia;* Rhombic-leaved alder, *A. rhombifolia;* Sea-side alder, *A. maritima.*

Elm.—Opaque-leaved elm, *Ulmus *opaca;* Thomas's elm, *U. ramosa.*

Hickory.—Small-fruited hickory, *Carya macrocarpa.*

Myrtle.—Inodorous candle-tree, *Myrica inodora.*

Plane-tree.—California button-wood, *Platanus ramosa.*

Poplar.—Narrow-leaved balsam poplar, *Populus angustifolia;* Cotton-wood, *P. lævigata.*

13. Choisy, *de Convolvulaceis dissertatio tertia complectens Cuscutarum hucusque cognitarum methodicam enumerationem et descriptionem, etc.* (in *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève,* Vol. 9, part 2, (1841–42,) pp. 261–288,) with 5 plates.—This full and finely illustrated monograph of a long neglected tribe of plants, is the more interesting to us, at the present moment, since Dr. Engelmann has been recently occupied in the study of the North American species, and published the results of his observations in the last (October) number of this Journal. Prof. Choisy describes thirty-eight species of Cuscuta from actual examination, twenty of which he has handsomely figured. Only four of this number are given as natives of extra-tropical North America, (although five others are derived from Mexico and New Spain,) viz. *C. Californica,* Chois. (California, *Douglas,*) and *C. glomerata, C. compacta,* and *C. Gronovii,* all new species from the United States. These species it is necessary to collate with those of Dr. Engelmann's monograph, especially as the former have the priority in publication; and we find, accordingly, that—

C. glomerata, *Chois. l. c. p.* 280, *t.* 4, *f.* 1 (St. Louis, Missouri, *Riehl, in herb. Reuter*) = Lepidanche Compositarum, *Engelm. monogr.*

C. compacta, *Chois. l. c. t.* 4, *f.* 2 (Alabama, S. Carolina, &c.) = Lepidanche adpressa, *Engelm. ined.;* a species which has only recently fallen under Dr. Engelmann's observation.

C. Gronovii, *Chois. l. c. t.* 4, *f.* 3 (Carolina to Boston) = C. vulgivaga, *Engelm. monogr.* (var. β.)

Prof. Choisy accords with Dr. Engelmann in retaining the name of *C. Americana* for the West Indian species. He also points out, and employs as an important distinctive character, the two states which the marcescent corolla assumes during the growth of the fructified ovary; either remaining persistent at its base, or elevated with it and covering its summit like a hood;—circumstances which have not escaped Dr. Engelmann's notice. According to the former, the entire capsule separates from the receptacle and falls to the ground unopened in the greater part of the genus; as Dr. Engelmann had remarked for the North American species. Prof. Choisy does not seem to have sufficiently regarded the characters upon which Dr. Engelmann founds his genus *Lepidanche;* but, as the sepals are nearly if not quite distinct in several species, and the flowers are often more or less bracteate, we suspect that this imbrication of bracts and sepals will not warrant the dismemberment of such a natural genus as Cuscuta. The other species described by Dr. Engelmann do not appear to have fallen under Prof. Choisy's observation. This author states that, so far as known, *Cuscutæ* are only parasitic on *Dicotyledons*, with the exception of a single instance, noticed by De Candolle, in which *C. minor* was found upon a grass: but Mr. Carey has detected the *C. Gronovii* on a species of *Leersia.* These parasites are most frequently found attached to *Compositæ* and *Leguminosæ*, perhaps because these are the largest and most widely diffused Exogenous families; but they avoid all plants which have acrid juices, such as *Ranunculaceæ* and *Umbelliferæ.*

14. Spring, *Monographie de la Famille des Lycopodiacées;* premiere partie, 1841, (in *Nouveaux Memoires de l'Académie Royale des Sciences et Belles-lettres de Bruxelles*, tom. xv, 1842,) pp. 110.—Prof. Spring, of the University of Liége, has been for several years occupied with the *Lycopodiaceæ*, and published in 1838, a *Beitrage zur Kentniss der Lycopodien* in the *Flora oder Botanische Zeitung* of Ratisbon, which was reproduced in the *Annales des Sciences Naturelles* for 1839. He has also recently elaborated the *Lycopodiaceæ* of Martius and Endlicher's *Flora Brasiliensis:* a work which we hope to notice at large in a future number of this Journal. This systematic monograph, so far as published, comprises the genus *Lycopodium*, as restricted by this author; who constitutes of *L. rupestre*, *L. apodum* and their allies, a distinct genus, with the name of *Selaginella*, and distinguishes the four genera of the order as follows:

1. Lycopodium. Antheridia unilocularia. Oophoridia nulla.
2. Selaginella. Antheridia unilocularia. Oophoridia 3–4-cocca.
3. Tmesipteris. Antheridia bilocularia. Oophoridia nulla.
4. Psilotum. Antheridia trilocularia. Oophoridia nulla.

Although he gives the name of *antheridia* and *oophoridia* to the two sorts of fructification in this family which are termed anthers and ovaries by some other botanists, he denies that they fulfill the office of these organs, or that the one in any way fecundates the other. This fecundation the author thinks impossible, for two reasons, (neither of which appear to us absolutely conclusive,) 1st, because there is no stigma or other conducting medium; and 2d, because the so-called anthers shed their contents in almost every instance (*Selaginella rupestris* and *S. spinulosa* being the only known exceptions) long before the so-called ovaries attain their full development. A stronger reason may be found in the fact, that the latter organs are not essential to propagation in this family, and are altogether wanting in three of the four genera which compose it. SPRING remarks, however, not only that these pollen-like grains burst like true pollen when thrown into water, with the escape of *fovillæ*, as Brown long ago remarked, but that he has also seen in *Selaginella*, (as Brown had remarked in *Psilotum*,) the production of *boyaux* from the grains, not unlike pollen-tubes. On the other hand, he has been able to confirm the fact of the germination of these grains in *Lycopodium*; but not in *Selaginella*, the only genus in which the two kinds of organs coëxist, and where a sexual antagonism may be supposed to take place: and in this case the globules or *spores* contained in the oophoridia have been observed to germinate and give rise to new plants. Germination, in both kinds of reproductive bodies, as in most other cryptogamic plants, takes place by the simple extension and growth of the whole spore, without the rupture of any integument.

Ninety-eight known, and three uncertain species of *Lycopodium* are here described; of which thirteen are natives of extra-tropical North America. Only two of this number are peculiar to this country, viz. *L. lucidulum* and *L. dendroideum*. It is remarkable that not only *L. complanatum* and *L. clavatum*, which are cosmopolites, but also *L. sabinæfolium*, which is otherwise restricted to British America, are found in Java. Our two principal southern species, *L. Carolinianum* and *L. alopecuroides*, both extend to Brazil; and the former is also a native of the Cape of Good Hope, Madagascar, &c.; and one of our Alpine species, *L. Selago*, is likewise antarctic. We append a conspectus of the North American species, with their geographical range.

Gen. LYCOPODIUM.

Sect. 1. Antheridiis sparsis, (Selago, *Dill.*)

§1. *Foliis undique conformibus.*

L. SELAGO, *Linn.*—Europe, Northern Asia, Boreal America, Azores, Peru, Falkland Islands, Van Diemen's Land.

§2. *Foliis fructigeris difformibus vel saltem minoribus.*

L. TAXIFOLIUM, *Swartz.*—Nootka Sound, (L. struthioloides, *Presl, rel, Hænk.*,) Mountains of the East and West Indies, St. Helena.

L. LUCIDULUM, *Michx.*—Pennsylvania to Newfoundland.

L. FUNIFORME, *Cham.*—California? Guadeloupe.

Sect. 2. Antheridiis in amenta congestis, (Lycopodium, *Dill.*—Lepidotis, *Beauv.*)

§3. *Foliis caulinis conformibus, caulem circa circum obsidentibus.*

**Amentis simplicibus: ramis sterilibus et fertilibus difformibus.*

L. INUNDATUM, *Linn.*—Europe and North America.

L. ALOPECUROIDES, *Linn.*—United States and Brazil.

***Amentis simplicibus: ramis conformibus.*

L. ANNOTINUM, *Linn.* (L. bryophyllum, *Presl.*)—Northern Europe, Asia, and North America.—A var. *β. pungens;* foliis erectis incurvis minus distincte serratis apice mucrone cartilagineo auctis, = L. reclinatum, *Michx.*, Newfoundland, Labrador, Greenland, Kamtschatka. [Also White Mountains of New Hampshire.—A. Gr.]

L. DENDROIDEUM, *Michx.* (L. obscurum, *Linn.*)—Newfoundland to the Mountains of Carolina and Oregon. [The author does not notice the two forms with which American botanists are familiar.]

L. SABINÆFOLIUM, *Wild.*—British America, Java!!

L. CLAVATUM, *Linn.* (L. integrifolium, *Hook.* L. tristachyum, *Nutt.* L. inflexum, *Swartz.* L. serpens, *Presl*, etc.)—Europe, Northern Asia, India, Java, Japan, South Africa, North America from Newfoundland to North West Coast, Mexico to Brazil.

§4. *Foliis caulinis dimorphis, caule vel compresso vel dorso nudo.*

**Caule dorso aphyllo, ramis humo adpressis.*

L. CAROLINIANUM, *Linn.* (L. repens, *Swartz.*)—New Jersey to Louisiana, Guadeloupe, Guiana, Brazil, Cape of Good Hope, Madagascar, Mauritius, etc. Ceylon?

***Ramis complanatis erectis.*

L. COMPLANATUM, *Linn.*—Europe, Northern Asia, India, Java, Newfoundland to Virginia, Jamaica, Mexico to Brazil.

L. ALPINUM, *Linn.*—Alpine Europe, Asia, and North America.

15. ENDLICHER, *Mantissa Botanica, sistens Generum Plantarum Supplementum secundum; auctore* STEPHANO ENDLICHER, (Vienna, 1842. pp. 114.)—This is the first of the occasional or annual supplements to his invaluable *Genera Plantarum*, which this author proposed to publish, as our readers are aware, (*Amer. Jour.* Vol. XLI, p. 373;) and is particularly interesting on account of the synopsis of the anatomical characters of fossil plants which it contains, and which are elaborated from materials furnished by M. Unger of Grâtz. Among the *Acrobrya* or *Acrogens*, several fossil families are established which have no representatives in the vegetation of the present world; viz. the order *Calamiteæ*, placed between Equisetaceæ and Ferns, *Psaronieæ* and *Stigmarieæ*, between the latter and Lycopodiaceæ, and the *Sigillarieæ*, which with *Lepidodendreæ*

follow the Lycopodiaceæ. Besides the extinct Coniferous forms, eighteen fossil Exogenous genera are separately described in an appendix. The next supplement will exhibit the latest views of systematic botanists respecting the lower Cryptogamic, or Thallogenous plants.

16. *Hooker's British Flora.*—We understand that the fifth edition of this deservedly popular Flora of Great Britain has just been published; in which, it is gratifying to learn, the Natural System has at length taken the place of the artificial Linnæan method hitherto employed in that work.

17. *Phillips's Mineralogy, new American edition.*—We learn from the editor that D. Ticknor, of Boston, has in press a new edition of Mr. Phillips's popular treatise on mineralogy, edited by Mr. F. Alger, of Boston.

MISCELLANIES.

DOMESTIC AND FOREIGN.

1. *Entomological Society of Pennsylvania.*—On the 23d of August, 1842, a number of gentlemen devoted to entomological pursuits, met in York, Penn., and established a society under the title above named.

A constitution and by-laws were adopted, and the following gentlemen elected officers. Dr. F. E. Melsheimer, President, Dover, York Co., Pa.; S. S. Haldeman, Esq., Vice President, Marietta, Lancaster Co., Pa.; Rev. D. Ziegler, Rec. Secretary, York, Pa.; Rev. Dr. J. G. Morris, Cor. Secretary, Baltimore, Md.; Rev. S. Oswald, Treasurer, York, Pa.

Dr. Morris read a portion of a descriptive catalogue of the diurnal Lepidoptera of the United States.

The President reported verbally that he is acquainted with the *larvæ* of upwards of five hundred species of our native Lepidoptera; that he had discovered many new species, and is still pursuing his investigations.

The Recording Secretary exhibited his *larvæ* nursery, of which he had a considerable number feeding.

The Vice President called the attention of the Society to several rare Coleoptera found in Pennsylvania, of which he exhibited specimens. Among these were *Capes cinerea*, S., *Cistela marginata*, Zieg., *Hoplia virens*, Hald., *Dicælus violaceus*, Bon.

The Recording Secretary laid on the table a description of three new species of Coleoptera—*Spercheus Americanus*, *Cistela marginata*, and *Pedilus flavicollis*.

The Corresponding Secretary gave a verbal account of a luminous insect captured in Maryland, but was unable to determine whether it was an apterous female of some coleopterous species or a larva. It was more

than two inches in length, and emitted, particularly from between the segments, a brilliant light. It died before undergoing any transformation.*

It was resolved that the Society prepare a catalogue (with specific characters of the new species) of the Coleoptera of the United States for publication.

At a meeting of the Society held in Baltimore, October 24th, considerable progress in the preparation of the catalogue was reported. Dr. T. W. Harris of Cambridge, Mass. was elected a corresponding member.

2. *Foot-Marks and other Artificial Impressions on Rocks:* in a letter from Prof. W. A. Adams to Prof. Silliman, dated Zanesville, Ohio, Aug. 6, 1842.—I was surprised to find in reading the 87th No. of the "American Journal of Science and Arts,"† that so many respectable authorities could be found on the affirmative side of the question, whether the human foot-prints found at St. Louis on the limestone rock are real impressions, and not works of art. The reasoning of Dr. Owen appears to me to be conclusive that they are artificial.

I have it in my power to communicate some facts tending still farther to illustrate this subject. In the spring of 1839, the high water in the Muskingum River caused a breach in the embankment of the canal at Zanesville. The embankment is constructed on the bank of the river, and is composed of earth, gravel, and fragments of sandstone, heaped upon similar rubbish placed there nearly forty years before, in erecting a mill-race around the natural falls in the river, the whole resting upon a sandstone rock, which constituted the bank of the stream. When this

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* I sent a description of this insect to a distinguished entomologist of Massachusetts, and he will excuse me for here inserting his reply without leave, as his observations on such subjects are justly considered valuable.—J. G. M.

"The beautiful luminous insect mentioned in your letter, has puzzled me much. The largest specimen that I have ever seen, measuring two inches and a half or more in length, was taken in Sutton, Mass., on the stump of a tree, by Dr. ——, who sent it to me in spirit, with an account of its splendid appearance when living. Since then, I have had several specimens alive, some found crawling on the grass in the evening, and others I have taken under stones. Abbot alludes to it in the preface to his "Insects of Georgia," and has sent me a figure of the insect with some remarks upon it. He supposes it to be mature; and from the form and structure of the mandibles, I thought it might be the female of *Phengodes plumosa*. All my specimens died without change, but perhaps if larvæ, they were too young and perished for want of their proper food. It cannot be the larva of an *Elater*, and it differs very materially from the known larvæ of *Lampyris* and *Telephorus*, two of which are mentioned in your letter. Were it not for the remarkable difference in the size of these luminous insects that have fallen under my own observation, I should think that they must be the wingless female of some species of *Lampyris* or *Phengodes*. I hope that you will be able to clear up the history of your insect."

† Vol. xliii, p. 14—Dr. Owen on human foot-prints in the limestone rock at St. Louis, Missouri, q. v.

embankment gave way, a large body of water passed over this rock, sweeping its surface clean, and leaving the rock exposed, and as it appeared before the first settlers began their improvements. Upon the surface of this sandstone rock, elevated only a few feet above the level of the river, and immediately upon its margin, were found *engraved* the impressions of two human foot-prints, and a number of turkey tracks. That these were the work of art is beyond all question; the human tracks were of the natural size, and accurately drawn; the turkey tracks were of large dimensions. The outlines of the human feet were made by a dotted line, as if a pointed chisel and mallet had been used, and an intaglio attempted by the same instrument. The whole surface within the outline was dotted over, barely removing the original surface of the rock; the form of the turkey tracks was made by a series of dots, and the whole seemed to have been left unfinished. These feet pointed south and down the river.

The discovery excited some curiosity at the time, and the impressions were seen by hundreds of people, who well remember them. Before the embankment was repaired, the part of the rock containing the foot-prints was quarried and broken into fragments. This rock had been covered with earth and loose stone for a period beyond the recollection of any of the inhabitants of the place; there can be no doubt that these sculptures were the work of the Indians. In addition to this, I have been informed that there is the impression of a single foot-print, in a rock situated on the bottom of Licking Creek, about seven miles below Newark, in this state; I have not seen this impression, but am well convinced of its existence; it is described as being occasionally under water. I am not informed of the kind of stone in which this impression is made; if now extant, it is near the narrows of Licking Creek, and in the vicinity of the "*Black hand*"—another interesting monument, which was destroyed in constructing the tow-path of the Ohio Canal.

The north bank of the creek, about seven miles below Newark, is formed of precipitous sandstone rock, forming a perpendicular wall about forty feet in height; upon the surface of this wall, which at this spot leaned a few degrees over its base, and about twenty feet from the bottom, was engraved a gigantic human hand: *the drawing was exact*, and *the proportion accurately preserved*. The hand, including the wrist, was about eight feet long; the outline was cut or scratched on the stone, and the whole space within the outline was stained or painted black; the color remained until the hand was destroyed, and might have endured for ages, as it was protected from rain by the projection of the rock. This hand had been known for more than fifty years before it was destroyed, and the place is yet designated as the "*Black hand.*" I believe these several marks or sculptures are identical with the celebrated and more perfect specimens found at St. Louis. It may perhaps be difficult, at this

period, to determine the purpose or meaning of these works of art. I have conversed with some old hunters and surveyors, who are well acquainted with the habits and character of the Indians; by them I am informed that it is the constant habit of the natives, when they quit an encampment, to leave some sign or hieroglyphic, designating the *course* they intend to pursue, and that the place is *abandoned*. For these purposes, what sign could be more appropriate than the prints of feet or *tracks?*

I am assured by an intelligent gentleman, who in early life was engaged in surveying the public lands in the west, that he has often seen such figures; sometimes a hand on a perpendicular surface, and print of feet where the surface is horizontal, as on the bark of a standing tree, or drawn with paint on a flat stone.

From these facts, is it reasonable to conjecture, that when a tribe have been compelled to abandon a permanent home, a more durable memorial of the event may be made by some labored sculpture on a permanent material, such as those found at St. Louis and here? The remarks of Dr. Owen are perfectly satisfactory in explaining the mode in which these works of art could be executed without the aid of iron.

We would add in further illustration of this subject, that Mr. Grey, in his journal of travels in Australia, Vol. I, p. 206,* gives a figure and the following description of a *head cut in sandstone rock*. "I was moving on when we observed the profile of a human face and head cut out in a sandstone rock which fronted the cave; this rock was so hard, that to have removed such a large portion of it with no better tool than a knife and hatchet made of stone, such as the Australian natives generally possess, would have been a work of very great labor. The head was two feet in length, and sixteen inches in breadth in the broadest part; the depth of the profile increased gradually from the edges where it was nothing, to the centre where it was an inch and a half; the ear was rather badly placed, but otherwise the whole of the work was good, and far beyond what a savage race could be supposed capable of executing. The only proof of antiquity that it bore about it was, that all the edges of the cutting were rounded and perfectly smooth, much more so than they could have been from any other cause than long exposure to atmospheric influences."—Eds.

3. *Dr. Draper's discovery of Latent Light, and of a curious class of Spectral Appearances connected with Photography.*—We find in the Nov. number of the London, Edinburgh and Dublin Philosophical Magazine, a letter from Dr. Draper to the editors, from which it appears that certain

* Journals of two Expeditions of Discovery in Northwestern and Western Australia, during the years 1837, '8, '9; by George Grey, Esq. 2 Vols. 8vo. London, 1841.

very curious observations on light, made by him from 1840 to the present time, have been credited to Dr. Möser of Königsberg, and an account given of them by Sir David Brewster, at the last meeting of the British Association.* Here is part of Dr. Draper's letter.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen—If there be a thing in which I have a disinclination to engage, it is controversy of a personal kind with scientific fellow laborers. But, as you well know, it ordinarily happens that there is no other gain to philosophers beyond the *mere credit* of their discoveries, they may be forgiven for reluctantly endeavoring to secure this their only reward.

I have recently returned from a long journey, undertaken for the purpose of making trials on the sunlight in lower latitudes, and am grieved to see in the reports that have reached this country of the Proceedings of the British Association, certain announcements, received from Professor Bessel, of phantoms which can be produced on surfaces by mercury vapor, by the breath, and other means,—*as though the thing were new.* Years ago, if you look into your own Journal, (Feb. 1840, p. 84; Sept. 1840, p. 218; Sept. 1841, pp. 198, 199;) you will find that I had published facts of the kind; spectral appearances, that could be revived on metals, glass, and other bodies, by the breath, by vapor of camphor, by mercury vapor, &c. The very purpose for which I described them, was the striking resemblance of some of them to Daguerreotope images. I I have repeatedly shown, that by placing a coin or any other object on iodized silver, *in the dark*, the vapor of mercury will bring out a representation of it. And in one of the papers just quoted, the condition under which camera images can be reproduced on a silver plate, even after the plate has been rubbed with rotten stone, is described.

I have further seen (Literary Gazette, July 23, 1842, Paris letter) that the fact that light becomes latent in bodies, after the manner of heat, was announced in France as a new and important discovery of Prof. Möser of Königsberg. In your own Journal, more than a year ago, you printed a long paper written by me on this very topic, (September, 1841, pp. 196, 204, 205, 206,) not merely announcing the fact, but giving rude estimates of the amounts: more exact numerical determinations I have *now* nearly ready for the press.† Yours truly, J. W. Draper.

University of New York, Sept. 26, 1842.

* The report of Prof. Möser's alleged discovery, from the Athenæum, is inserted at page 159 of our present number.—Eds. Am. Jour.

† Since the above was in type, we have received the December number of the London, Edinburgh and Dublin Philosophical Magazine, and find much new, interesting and important matter relative to this subject, from Dr. Draper and Mr. Hunt, (pp. 453, 462;) also in the Athenæum for Nov. 19, an abstract of Mr. Hunt's paper, and further accounts of Prof. Möser's observations. We hope in our next to give the details of a discovery which promises to be of as much interest as the original observations of Daguerre.—Eds. Am. Jour.

Photography.—We copy the following from the same letter of Prof. Draper from which the foregoing is taken :—

The accompanying photographic impression of the solar spectrum, which I will thank you to give to Sir John Herschel, was obtained in the south of Virginia : probably you can make nothing like it in England, the sunlight here in New York wholly fails to give any such result. It proves, that under a brilliant sun, there is a class of rays commencing precisely at the termination of the blue, and extending beyond the extreme red, which totally and perfectly arrest the action of the light of the sky. This impression was obtained when the thermometer was 96° Fah. in the shade, and the negative rays seem almost as effective in protecting, as the blue rays are in decomposing iodide of silver.

The most remarkable part of the phenomenon is, that the same class of rays makes its appearance again beyond the extreme lavender ray. Sir J. Herschel has already stated, in the case of bromide of silver, that these negative rays exist low down in the spectrum. This specimen, however, proves that they exist at both ends, and do not at all depend on the refrangibility. It was obtained with yellow iodide of silver, Daguerre's preparation, the time of exposure to the sun fifteen minutes.

In this impression, six different kinds of action may be distinctly traced by the different effects produced on the mercurial amalgam. These, commencing with the most refrangible rays, may be enumerated as follows :—1st, protecting rays; 2d, rays that whiten ; 3d, rays that blacken ; 4th, rays that whiten intensely ; 5th, rays that whiten very feebly ; 6th, protecting rays.

It is obvious we could obtain negative photographs by the Daguerreotype process by absorbing all the rays coming from natural objects, except the red, orange, yellow, and green, allowing at the same time diffused daylight to act on the plate.

This constitutes a great improvement in the art of photography, because it permits its application in a negative way to landscapes. In the original French plan the most luminous rays are those that have least effect, whilst the sombre blue and violet rays produce all the action. Pictures, produced in that way, never can imitate the order of light and shadow in a colored landscape.

If it should prove that the sunlight in tropical regions differs intrinsically from ours, it would be a very interesting physical fact. There are strong reasons to believe it is so. The Chevalier Fredrichstal, who travelled in Central America for the Prussian government, found very long exposures in the camera needful to procure impressions of the ruined monuments of the deserted cities existing there. This was not due to any defect in his lens ; it was a French achromatic, and I tried it in this city with him before his departure. The proofs which he obtained, and which he did me the favor to show me on his return, had a very remarka-

ble aspect. More recently, in the same country, other competent travellers have experienced like difficulties, and, as I am informed, failed to get any impressions whatever. Are these difficulties due to the antagonizing action of the negative rays upon the positive?

4. *On the Salt Steppe south of Orenburg, and on a remarkable Freezing Cavern;* by RODERICK IMPEY MURCHISON, Esq., Pres. G. S.—This salt steppe is distinguished from many of those which are interposed between the Ouralsk and the Volga, or are situated on the Siberian side of the Ural Mountains, by consisting not of an uniform flat resembling the bed of a dried-up sea, but of wide undulations and distantly separated low ridges; nevertheless it is, Mr. Murchison states, a true steppe, being devoid of trees and little irrigated by streams. The surface consists of gypseous marls and sands, considered by the author to be of the age of the zechstein,* and it is pierced in the neighborhood of the imperial establishment of Illetzkaya Zatchita by small pyramids of rock salt. These protruding masses attracted the attention of the Kirghiss long before the country was colonized by the Russians, but it is only during a short period that the great subjacent bed has been extensively worked. The principal quarries, exposed to open day, are situated immediately south of the establishment, and have a length of three hundred paces, with a breadth of two hundred, and a depth of forty feet. The mass of salt thus exposed, is of great purity, the only extraneous ingredient being gypsum, distantly distributed in minute filaments. At first sight the salt seems to be horizontally stratified, but this apparent structure, Mr. Murchison states, is owing to the mineral being extracted in large parallelopipedal blocks twelve feet long, three feet deep and three wide. On the side where the quarry was first worked, the cuttings presented, in consequence of the action of the weather, a vertical face as smooth as glass, but at its base there was a black cavern formed by the water which accumulates at certain periods of the year, and from its roof hung saline stalactites. The entire range of this bed of salt is not known, but the mass has been ascertained to extend two versts in one direction, and Mr. Murchison is of opinion that it constitutes the subsoil of a very large area; its entire thickness also does not appear to have been determined, but it is stated to exceed one hundred feet. The upper surface of the deposit is very irregular, penetrating, in some places, as already mentioned, the overlying sands and marls.

* His extensive surveys of Russia have convinced Mr. Murchison that rock-salt and salt springs occur in all the lower sedimentary rocks of that empire, from great depths below the Devonian or old red sandstone system to the zechstein and the overlying marls and sandstones.

In consequence of the salt occurring at so small a depth, every pool supplied with springs from below is affected by it;* and one of them used by the inhabitants as a bath is so highly charged with saline contents that there is a difficulty in keeping the body submerged, and the skin on leaving the pool is encrusted with salt. This brine swarms with animalcules.

Mr. Murchison then describes the freezing cavern and the phenomena exhibited by it. The cave is situated at the southern base of a hillock of gypsum, at the eastern end of the village connected with the imperial establishment; and it is one of a series of apparently, for the greater part, natural hollows, used by the peasantry for cellars or stores. The cave in question is, however, the only one which possesses the singular property of being partially filled with ice in summer and of being destitute of it in winter. "Standing on the heated ground and under a broiling sun, I shall never forget," says the author, "my astonishment when the woman to whom the cavern belonged unlocked a frail door, and a volume of air so piercingly keen struck the legs and feet that we were glad to rush into a cold bath in front of us to equalize the effect." Three or four feet within the door and on a level with the village street, beer and quash were half frozen. A little further the narrow chasm opened into a vault fifteen feet high, ten paces long, and from seven to eight wide, which seemed to send off irregular fissures into the body of the hillock. The whole of the roof and sides were hung with solid undripping icicles, and the floor was covered with hard snow, ice, or frozen earth. During the winter all these phenomena disappear, and when the external air is very cold and all the country is frozen up, the temperature of the cave is such that the Russians state they could sleep in it without their sheep-skins.—*Lond. Edin. and Dub. Phil. Mag. for Nov.* 1842.

5. *Extracts from a letter addressed by Sir J. Herschel, Bart., F. G. S., to Mr. Murchison, explanatory of the Phenomena of the Freezing Cave of Illetzkaya Zatchita.*—"That the cold in ice-caves (several of which are alluded to in a part of this letter not published) does NOT *arise from evaporation*, is, I think, too obvious to need insisting on. It is equally impossible that it can arise from condensation of vapor, which produces heat, not cold. When the cold (by contrast with the external air, i. e. the difference of temperature) is greatest, the reverse process is going on. Caves in moderately free communication with the air are dry and (to the feelings) warm in winter, wet or damp and cold in summer. And from the general course of this law I do not consider even your Orenburg caves

* The abundance of these brine-springs in various parts of Russia must lead, the author says, to the abandonment of Pallas's hypothesis, that the saline pools and lakes are the residue of former Caspians; though he admits that some of the vast low steppes of the South formed the bottom of a former condition of the existing Caspian.

exempt, since however apparently *arid* the external air at 120° Fahr.! may be, the moisture in it may yet be in excess and tending to deposition, when the same air is cooled down to many degrees beneath the freezing point.

"The data wanting in the case of your Orenburg cave, are *the mean temperature of every month in the year of the air*, and of thermometers buried say a foot deep, on two or three points of the surface of the hill, which if I understand you right is of gypsum and of small elevation. I do not remember the winter temperature of Orenburg, but for Catherinenburg (only 5° north of Orenburg) the temperatures are given in Kuppfer's reports of the returns from the Russian magnetic observatories. If any thing similar obtains at Orenburg I see no difficulty in explaining your phenomenon. Rejecting diurnal fluctuations and confining ourselves to a single summer wave of heat propagated downwards alternately with a single winter wave of cold, every point at the interior of an insulated hill rising above the level plain will be invaded by these waves in succession, (converging towards the centre in the form of shells similar to the external surface,) at times which will deviate further from mid-winter and mid-summer the deeper the point is in the interior, so that at *certain depths* in the interior, the cold-wave will arrive at mid-summer and the heat-wave in mid-winter. A cave (if not very wide-mouthed and very *airy*) penetrating to such a point will have its temperature determined by that of the solid rock which forms its walls, and will of course be so alternately heated and cooled. As the south side of the hill is *sunned* and the north *not*, the summer wave will be more intense on that side and the winter less so; and thus, though the *form* of the wave will still generally correspond with that of the hill, their *intensity* will vary at different points of each wave-surface. The analogy of *waves* is not strictly that of the progress of heat in solids, but nearly enough so for my present purpose.

"The mean temperature for the three winter months, December, January, February, and the three summer months, June, July, August, for the years 1836, '7, '8, and the mean of the year, are for Catherinenburg as follows:—

Years.	Winter.	Summer.	Annual mean.
1836,	−10°.93 R.	+11°.90 R.	+1°.22 R.
1837,	−12°.90	+12°.93	+0°.30
1838,	−12°.37	+12°.37	+0°.60
Mean,	−12°.07 R.	+12°.40 R.	+0°.70 R.
	+ 4°.83 Fah.	+59°.9 Fah.	+33°.57 Fah.

"The means of the intermediate months are almost exactly that of the whole year, and the temperature during the three winter as well as the three summer months most remarkably uniform.

"This is precisely that distribution of temperature over time which ought under such circumstances to give rise to well defined and intense waves of heat and cold; and I have little doubt therefore that this is the true explanation of your phenomenon.

"I should observe, that in the recorded observations of the Catherinenburg observatory, the temperatures are observed two-hourly, from eight, A. M. to ten P. M., and not at night. The mean monthly temperatures are thence concluded by a formula which I am not very well satisfied with; but the error, if any, so introduced, must be far too trifling to affect this argument. The works whence the above data are obtained, are '*Observations Météorologiques et Magnétiques faites dans l'intérieur de l'Empire de Russie,*' and '*Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines de Russie,*' works which we owe to the munificence of the Russian government, and which it is satisfactory to find thus early affording proofs of utility to science in explaining what certainly might be regarded as a somewhat puzzling phenomenon, as it is one highly worthy of being further studied and being made the subject of exact thermometric researches on the spot, and wherever else any thing similar occurs."—*Lond. Ed. and Dub. Phil. Mag. for Nov.* 1842.

6. *Further observations on the Meteors of August* 10, 1842.—Such partial observations as the weather at this place permitted, were published in the last number of this Journal, (Vol. XLIII, p. 377,) from which it was confidently inferred that the meteoric display of the tenth of August recurred the present year with undiminished numbers. Various European observations which have reached us, confirm the justness of this conclusion.

At *Vienna*, according to a letter from Mr. C. J. Littrow, (published in the *Literary Age*, Vol. I, p. 22, Dec. 14, 1842,) there were observed by this astronomer and his assistants, on the night of the 10th of August last, *seven hundred and seventy-nine* shooting stars. The number of observers is not mentioned.

The following information has been obligingly communicated to me by M. Quetelet of Brussels.

At *Parma*, in Italy, observations were made by M. Colla. Aug. 9, from 9h. 3m. to 15h. 34m., 252 shooting stars were noted; on the 10th, from 8h. 36m. to 15h. 20m., 490 were seen. The number of observers is not stated.

The following table contains the results of the two nights taken together.

From	8h. 36m.	to	9, . . .	3	meteors.
"	9	"	10, . . .	37	"
"	10	"	11, . . .	96	"
"	11	"	12, . . .	84	"

From 12h.	to 13,	. . . .	127 meteors.
" 13	" 14,	. . . .	175 "
" 14	" 15,	. . . .	170 "
" 15	" 15½,	. . . .	50 "

At *Berlin*, there were noted by M. Boguslawski, on the night of August 9th, from 9h. 14m. to 14h. 48m., 401 meteors; on the following night, from 9h. 7m. to 15h., 783. Number of observers not stated.

At *Brussels*, observations were made by M. Quetelet and an assistant, in concert with M. Boguslawski at Berlin, for the determination of differences of longitude. On the night of the 9th, from 9h. 30m. to 13h. 30m., 150 meteors were noted by two observers. On the night of the 10th, during 136 minutes, between 10h. and 14h. 26m., 167 meteors were noted. Part of this time there was only one observer. On the night of the 11th, during 120 minutes, between 10h. and 13h. 30m., 110 meteors were noted. There was on these occasions a decided radiating point, in the vicinity of the constellation Perseus, as in former years. The moon of course rendered invisible many meteors, which would have been seen in her absence; and many more must have been lost, for want of a sufficient number of observers. E. C. H.

7. *Meteoric Observations in November*, 1842.—Although it was not expected that a recurrence of the meteoric display of the thirteenth of November would be observed the present year, yet it was deemed desirable to keep watch at that period. The morning of the thirteenth proved to be so cloudy as to prevent observation. On the morning of the 14th, Mr. Joseph S. Hubbard and myself watched in the open air for one hour, commencing at three o'clock. During this time we saw 46 shooting stars, (25 in the W. and 21 in the E.,) the sky being partially obscured by clouds. At 4 o'clock the sky had become so cloudy that it seemed useless to watch longer. The meteors were not remarkable for brilliancy or for uniformity of motion; about two thirds of them obeying the usual radiant tolerably well. The Zodiacal Light was, as is common at this season of the year, very conspicuous, extending upward to α Leonis, and as seen by indirect glances, nearly as far as the nebula in Cancer.

The morning of the 15th was clear. Mr. Francis Bradley, watching alone in the northern sky for twenty minutes, observed *seven* meteors. On the mornings of the 16th and 17th the sky was overcast. On the evening of the 21st there was a considerable display of the *Aurora Borealis*. No certain indication of this phenomenon had been previously seen here since the 29th of September.

From the foregoing observations, it was concluded that there was this year at this place no very decided recurrence of the November meteoric display.

Rochester, N. Y.—A notice signed W. G., and published in the Western New Yorker, Rochester, Nov. 22, states that at that place, "the evenings of the 12th, 13th and 14th were covered with clouds, except at intervals, and during these a bright moon prevented observation. On the morning of the 15th, during the short interval that elapsed between the setting of the moon and daylight, a large number were observed—in one instance no less than six or seven at a time, and scattering ones continued to fall until the light of the morning hid them from view. They all seemed to start from about the same point in the heavens as in former observations, viz. in the N. E., at an elevation of about 60°; and their movement was to the S. E., though there were a few exceptions. At the rate they fell during the few minutes they were observed, the number would be from 150 to 200 in an hour."

It must be remembered, however, that it is not safe to infer the hourly number of meteors, from observations continued only a few minutes. It often happens that after a barren interval of many minutes, two, three or more meteors will appear in rapid succession, to be followed by another barren interval of considerable duration. E. C. H.

8. *Meteoric Observations in December*, 1842.—From the 4th to the 13th of this month, the weather here has, with few exceptions, been exceedingly unfavorable for celestial observation. Since the year 1838, the meteoric display of December 6-7 has apparently failed; but the weather has scarcely permitted us here to determine the question as regards the present year. The evenings of Dec. 4, 5, 6, 8, 11, 12, and 13, were overcast. On the 7th and 9th I watched alone soon after dark, for about half an hour, and saw each time only two or three meteors. The moon was shining, and the sky partly obscured. On the evening of the 10th, Mr. Bradley and myself watched an hour, ending 7h. 15m. P. M., and saw but three meteors. During this hour the moon was faintly shining, and the sky about two thirds obscured. The mornings of the 10th, 11th, and 12th were overcast.

In a letter published in *L'Institut*, No. 422, M. Colla states that at Parma, the number of shooting stars visible Dec. 6–7, 1841, was uncommonly small. On the nights of the 10th and 11th, however, he observed at that place, an unusually large number of these meteors. In half an hour during the night of the 11th, he counted in the northern quarter, 23 very brilliant meteors, nearly all of them having luminous trains. Their general direction was from S. to N. An assistant reported that the next morning meteors pursuing the same direction were uncommonly abundant. A similar phenomenon was, according to M. Colla, remarked at Parma in 1833 and 1836, on the same night, viz. Dec. 11–12.

In the same letter M. Colla mentions several other remarkable occurrences of shooting stars in 1842, viz. July 22; Aug. 9–12, 24 or 25; Sept. 10,

18, 19, 20; Oct. 10, 17, 25; Nov. 19. He does not state the number of meteors actually counted on these several occasions. With the exception of Aug. 9–12, perhaps none of these occurrences would be considered remarkable, if we assume *thirty per hour* to be the mean number of shooting stars visible at one place. E. C. H.

9. *Solar Eclipse of July* 8, 1842.—In Vol. XLII, (pp. 175—181,) we published a paper by R. T. Paine, Esq., of Boston, containing an elaborate computation of the phases of the total eclipse of the sun of July 8, 1842, for several places on the continent of Europe. The eclipse was carefully observed by numerous eminent astronomers. We are much gratified to learn by a letter from Mr. C. J. Littrow, Director of the Imperial Observatory at Vienna, to Mr. Paine, (*Literary Age*, Philad. Vol. I, p. 22,) that the computations of the latter corresponded remarkably well with the observations. Mr. L. remarks: "The observations accord better with your calculations than with those of any European astronomer. The comparison is as follows:

	h.	*m.*	*s.*			*s.*
Beginning of eclipse,	5	51	51.9	Diff. from your predic.		+33.9
" total darkness,	6	49	24.9	"	"	+26.9
End "	6	51	21.9	"	"	+26.9
Duration "		1	57.0	"	"	0.0
End of eclipse,	7	53	55.9	"	"	+19.9

"These differences are so small and so regular, as to leave nothing to be desired, and I should be glad to know the method which you have used. Accordingly, I take the liberty of offering you as a medium of publication of your calculations, the Annals of our Observatory."

10. *Discovery of a Comet.*—At 7 P. M., October 28, 1842, a telescopic comet was discovered in the constellation *Draco*, by M. Laugier, Astronomer at the Paris Observatory. It was extremely faint, and without any appearance of tail. At 10h. 10m. P. M., Paris mean time, its R. A. was 16h. 41m., N dec. 68° 44′. In six hours the R. A. increased 3m. 34s. and the declination decreased 20′. M. Laugier observed it again Nov. 2, 4, 5, and computed its parabolic elements. Pingré's *Cometographie* mentions a comet seen in China, A. D. 1301, the elements of which, calculated from the observations of the Chinese, accord in a remarkable manner with the results of the new calculation. It is therefore possible that M. Laugier has had the good fortune to record the second passage of a comet whose period occupies more than five hundred years. On the 14th of November M. Laugier made to the Academy of Sciences of Paris a further communication on the comet, announcing that on the next day it would be at its nearest point to the earth, from which however it would then be distant more than seven millions of leagues.—*Lond. Athen. Nov.* 1842.

11. *Prof. Espy's Meteorological Enterprise.*—It is probably known to most cultivators of meteorology in America, that Prof. J. P. Espy, formerly of Philadelphia, has become connected with one of the departments of the War Office at Washington City. He has published blank forms for recording meteorological phenomena, and is ready to distribute them gratis to all who will take the trouble to keep a register and return it monthly to him, directed to the Adjutant General's Office, endorsed on one corner "Meteorology." His object is to collect the greatest possible amount of meteorological matter, and particularly records of violent storms of wind and rain. His position at the seat of government, with the powerful aid of the War Office, and the regular monthly returns of the meteorological registers kept at all our military posts, as well as those forwarded by colleges, academies, schools, and individuals, will enable him to command more extensive and valuable materials than have ever been in the possession of any single observer in this country.

We strongly recommend to all whose observations are not promised in another channel, to throw their efforts into the common cause. Copies of all registers kept for other purposes, are also most earnestly desired.

From such a mass of materials as can in this way be collected, great results must be expected, when we shall be able to solve the details by some generally accepted and unobjectionable mode. For by whatever *theory* they may be claimed, the *facts remain*, and in the end can sustain only the truth.

12. *Copal.**—This resin is found upon the coast of Africa. There are no trees in the vicinity, nor any thing to indicate that there ever were any. It is found a few inches below the surface of the ground, in beds resembling lava. The natives who gather it can give no account of its origin. When taken from the bed, the gum is covered with a black earthy substance, which nothing can remove but the strongest lye. As the only establishment known to exist for cleansing it, is at Salem, Mass., all the gum brought to this country is sent there. It is placed in vast vats, into which strong lye is poured, and after remaining there some days, it is removed, spread upon boards, and dried in the sun. The action of a stiff brush then removes the coating, and renders it fit for use. It is then assorted, the clear (which is the first quality) separated from the dark and the spotted; and it is then packed in boxes, and sent to all parts of the world.

By removing the native coating, the gum is left with a pale gold color. But by cutting with a knife through the second coating, a brilliant surface is presented, that nearly equals the brilliancy of precious stones. In many pieces of gum insects are found, large, perfect and beautiful; also fluids, some transparent, some colored.

* This is an anonymous MS. communication to the Editors.

13. *Notice of the Discovery of an Electrical Fish on our coast:* in a letter from D. Humphreys Storer, M. D. to the Editors, dated Boston, Dec. 23, 1842.—A species of Ray possessing electrical powers has been known to the fishermen of Cape Cod and New York for many years, and called by them the *cramp-fish* or *numb-fish*. Mitchill in his paper on the "Fishes of New York," contained in the first volume of the Transactions of the Literary and Philosophical Society of New York, refers to this species; he had never seen it, but, from the facts he was enabled to collect respecting it, he supposed it to be identical with the European species, "*Raia torpedo*," and as such, introduces it into his memoir. In my Report on the Fishes of Massachusetts, I merely observed that a Torpedo was found on the coast of Cape Cod, but being unable to procure a specimen, I could not identify it. I have had the good fortune to procure a fine specimen within the last month, which was captured at Wellfleet; it was four feet and two inches in length, and proves to be the *Torpedo nobiliana*, Buonaparte. It agrees perfectly with Mr. Thompson's description, in the fifth volume of the Annals of Natural History, of a specimen taken on the Irish coast in 1838.

As some time may elapse before I can publish a contemplated paper on our fishes, I would avail myself of your valuable Journal to make the above mentioned fact known to ichthyologists.

14. *To detect minute quantities of Arsenic and Antimony.*—Dr. Brett, wishing to ascertain the minimum quantity of arsenic that can be detected by Marsh's process, has by experimenting on an alloy of zinc and arsenic, been able to detect one part in 5000; and by dissolving the arsenious acid in water, the minimum quantity of arsenic is =.00469 part. The sulphuric acid and zinc were previously purified.

With an alloy of zinc and antimony, 1 part antimony to 13000 zinc, would be detected by the metallic stain. In examining the sesquioxide of antimony under the form of tartar emetic in solution, the minimum quantity was =.00522 gr. When the quantity of the metals is very minute, the metallic stains are not apparent on white porcelain for some minutes, and the deposition, with a considerable volume of the gas, may even go on for fifteen or twenty minutes. The color of the arsenical stain is brown, and its intensity and that of the metallic stain increases with the quantity. The antimonial stains are of a dark, almost black color, except when approaching the minimum quantity of antimony, then the color resembles the brown of arsenical stains. When the hydrurets of arsenic and antimony are mixed, then (unless excessively minute) each exhibits its characteristic color, the brown arsenical stain appearing around the dark and almost black antimonial stain, owing probably to the greater volatility of the arsenic.—*Lond. Edin. and Dub. Jour. May*, 1842.

15. *Mineralogical systems.*—The following is the substance of the remarks by Prof. Whewell in his History of the Inductive Sciences, Vol. III, p. 526. In speaking of Mohs's system and of its failure, he says, the design of giving to all minerals names in accordance with his system was too bold to succeed. A new nomenclature truly was needed; but an improved classification ought to have furnished an improved nomenclature, as in botany by Linnæus. Mohs had not prepared his verbal novelties with the temperance and skill of the great botanical reformer. He called on mineralogists to change the name of almost every mineral with which they were acquainted, and the proposed appellations were mostly of a cumbrous form. Berzelius contrived a purely chemical system, on the *electro-positive* principle, both as to minerals and elements. Mitscherlich's *isomorphism* showed that minerals with very different electro-positive elements could not be distinguished. Berzelius then assumed the *electro-negative* character of the formative element as the basis of his system, viz. all sulphurets together, all the oxides, and all the sulphates. Gmelin, in 1825, assumed the same fundamental principle, and took account of the number of atoms or proportions; e. g. the silicates were simple, double —quintuple, as pitchstone—sextuple, as pearlstone, &c. Nordenskiöld assumed the same bases, but regarded also crystalline form. Beudant proceeded on the *electro-negative* principle, and on Ampère's circular arrangement of elementary substances.

Such schemes exhibit rather a play of mere logical faculty, exercising itself on assumed principles, than any attempt at the real interpretation of nature. Both these attempts by Mohs and Berzelius failed because there was no coincidence between them. A chemical arrangement and a natural history system should agree, and thus verify each other. The interior and the exterior systems should be *type* and *antitype, else they are not successful.*

He then proceeds to give other reasons. Berzelius's electro-*positive* principle was soon found false. Why should not the electro-*negative* prove so also? The former is isomorphous and the latter too; e. g. arsenic and phosphoric acids. How do we know that the external properties depend on electrical properties? The composition of all minerals, even of those most analyzed, is not yet certain; and this ancient notion of the "composition of a species," is unsettled by *isomorphism.* The constancy of the angles also is rendered doubtful by *plesiomorphism.* The *optical* properties, yet so imperfectly known, are somewhat arbitrary and capricious. The *chemical* and *optical* mineralogists have been obliged frequently of late to separate species which had been united, and to bring together those which had been divided. Every thing shows that classification *is still to begin.* A *fixity* of characters for the foundation of species is *not yet known.* The natural history classifiers assume that they can discover the *relative value* and *importance of those characters.* The

grouping of species into a genus, or genera into an order, is not by *definite rules*, but by a *latent talent of appreciation*—a sort of *classifying instinct*. These are *purists*. Naumann in 1828 adopted a mixed system, the chemical and natural history. He made *haloides*, *salts of oxides*, *unmetallic* and *metallic*, and these *hydrous* and *anhydrous*, which divisions give good natural groups.

The *siliceous minerals* are the most difficult to arrange. He calls them *silicides*—metallic and unmetallic, amphoteric—and again hydrous and anhydrous. This is a good basis for future researches. Natural history began to make its appearance in systems of chemistry. The combination of the chemical, crystallographical, and optical properties, into some lofty generalizations, is probably a triumph reserved for future and distant years; for a complete geological survey of the whole earth is requisite as a foundation for sound theory.

16. *Atomic Weights of Elements.*—MM. Marchand and Erdmann are at present engaged in a series of researches, which seem to prove that Prout's idea, that all atomic weights are multiples of that of hydrogen, is correct. They have as yet examined only the following bodies:

Oxygen	= 100. . . 8	Calcium	= 250 . . 20
Hydrogen	= 12.5 . . 1	Chlorine	= 450 . . 36
Carbon	= 75. . . 6	Silver	= 1250 . . 100
Nitrogen	= 175. . . 14	Lead	= 1300 . . 104

Phil. Mag. Nov. 1842.

17. *Sugar in the Stalks of Maize.*—Dr. Pallas, principal physician of the military hospital of St. Omers, addressed to the French Academy of Science during the past summer a memoir, in which he attempts to establish the following propositions—viz. 1st, that the stems of Indian corn contain little or no sugar before flowering: 2d, that just at the epoch of flowering only a mere trace of sugar can be extracted from them: 3d, that the same stems, at from twenty to twenty five days after flowering, while the grain is yet milky, contain one per cent. of crystallizable sugar: 4th, that still later, when the grain is perfectly ripe, the stems yield two per cent. of coarse sugar, besides four per cent. of rich molasses. The commission to whom this memoir was referred, (M. Biot, reporter,) however, very properly discredit these results, so contradictory not only to our theoretical views, but also to all former observations; as, for example, those of Prof. Burger, who affirms that the sugar is most abundant in the juice of the stem of maize immediately after the expansion of the flowers, and that it diminishes as the grain is matured. The commission states that the experiments of Dr Pallas on this subject were so loosely conducted as to afford no results worthy of confidence.—*Comptes Rendus, August* 29, 1842.

18. *Correction of the statement concerning an alleged "Shower of red matter like blood and muscle."*—We published, at the request of Dr. Troost, (in this Journal, Vol. XLI, pp. 403, 404,) an account of what was believed by him and other competent judges to have been a shower or fall of red blood and muscle from the atmosphere. Our own belief in the facts was never full; not doubting that Dr. Troost had given a faithful account of the supposed phenomenon as related to him; but the event seemed so improbable, that we always feared some foul play on the part of the informers. Not long after our account was published, it was announced in the newspapers that the whole affair was a hoax devised by the negroes, who pretended to have seen the shower, for the sake of practising on the credulity of their masters. They had scattered the decaying flesh of a dead hog over the tobacco leaves. We are led at this late day to call attention again to what ought to be forgotten, because we are reminded by a notice in L'Institut for May 15, 1842, that we have never contradicted the statement in our pages, and that it therefore has been quoted by the editor of L'Institut as authority against a true statement of the facts, contained in a letter from M. de Castlenau, published in "Le National," a public journal. We regret that our neglect should have been quoted in support of error, and beg the editor of L'Institut to correct the statement.

19. *Obituary.*—It is our painful duty to record the death of Baron Louis Lederer, Consul General for the Austrian States. His death took place on the 22d of December, 1842, at his residence in New York. Baron Lederer has been for many years a resident in this country, and has been long known as a zealous mineralogist. The death of this excellent man will be deeply felt by all who knew him, and particularly by his mineralogical friends in this country, who are under many obligations to him for his uniform kindness, and the important aid he was able to render them by means of his close connection with the imperial cabinet at Vienna. His collection of foreign minerals was purchased three or four years since by the University of Michigan, and his collection of American specimens in mineralogy, which is believed to be unique in its fullness, was offered by him for sale just before his death.

We had engaged to Baron Lederer, to publish in this number his own specification of the contents of his American cabinet, in relation to its sale in the United States; but not knowing what may be the views of his successors and representatives, we forbear until farther instructed.

APPENDIX

TO THE AMERICAN JOURNAL OF SCIENCE AND ARTS, VOL. XLIV, NO. I.

VINDICATION

OF CLAIMS

TO CERTAIN INVENTIONS AND IMPROVEMENTS

IN THE

GRAPHIC ART.

BY ASA SPENCER.

Editorial Remark.—This communication came too late for insertion in the body of this number. By Mr. Spencer's desire, it is added in the present form. Although we are averse to controversy, we cannot object to the vindication of the just claims of individuals, especially when a discussion is conducted as in the present case, by both parties with perfect courtesy, and a disposition to do, as well as to claim justice.

Philadelphia, July 20, 1842.

Gentlemen—A few days since an acquaintance directed my attention to your very valuable work, entitled "A Manual of Coins and Bullion," published at the Assay Office of the Mint.

On looking over the sixth chapter, which contains a description of the plates, I was surprised to see in a work from such an enlightened quarter, so erroneous on account of the origin and progress of the medal-ruling machine, by which it is made to appear that the invention and improvement of the machine, belong exclusively to two gentlemen of the Mint, and I am spoken of as a mere copier from Mr. Gobrecht.

I will endeavor to give a plain and simple history of the whole affair. In the fall of 1816, I came to this city with Mr. Jacob

Perkins, whose object was, in conjunction with Messrs. Murray, Fairman & Co., to introduce into the art of bank-note engraving, a new and original style of work, the production of a machine invented by me, and called the Geometric Lathe. Mr. Gobrecht was, at that time, employed in the same establishment, which afforded us frequent opportunities of conversing on subjects connected with the business. He talked much concerning ruling machines—had a great desire to possess one, and told me he had attempted to make one, to be moved by a screw, but that it failed from the imperfection of the screw. He thought, however, that by the aid of such means and implements as were then in my possession, he could be furnished with a screw of sufficient accuracy for the purpose. I undertook to assist him; but had not proceeded far when I became convinced, that the plan we were upon would not answer. Having, however, embarked in the enterprise, I was unwilling to give it up, and began immediately to study some other plan; when a modification of the wheel and axle, like that of the plating mill, was fixed upon by me. I then made known to Mr. Gobrecht my entire want of faith in his old plan, and described to him my new one, in which I had the fullest confidence. He, however, was not pleased with it, and seemed loath to give up his own, but after consulting with Mr. Perkins, who gave a decided preference to my plan, and spoke in the highest terms of it, he became anxious to have the first machine on the new plan. I undertook it for him; my other engagements made it necessary, that I should get it up in the simplest form, and with the least possible labor. I had previously to this furnished Mr. Gobrecht, at his request, with a very simple plan for ruling waved lines, taken from a movement in the Rose Engine, and easily attached to any ruling machine, consisting of an arm or lever, with a point or touch, as it is called in the Rose Engine, resting on a waved surface and kept in contact with it by a suitable weight or spring. This touch being connected with the slide which carries the etching point, and made to move over the waved surface or model, a vibratory motion thus caused was, by joints adjusted for the purpose, communicated to the etching point.

Thus the machine was placed in Mr. Gobrecht's hands, with the exception, merely, of the waved surface or model, which was left for him to supply, to suit himself. This last appendage to

the machine, (taken from the Rose Engine,) had not been in operation when he took it away. I have no recollection of having heard Mr. Gobrecht speak of the machine afterwards.

Soon after the delivery of it, Mr. Gobrecht exhibited his representation of the head of the Emperor Alexander. The novelty of its appearance attracted great attention, and was a subject of eager inquiry among the curious, as to how it was done, but Mr. Gobrecht maintained a rigid silence on the subject. It remained a profound and guarded secret, until I had prepared a machine of the same kind for my own use, and taken it to London, whither my business called me, and where it was finished and put in operation by me; and then and there, for the first time, I saw the appendage for ruling waved lines, in action, and was surprised to see with what truth and accuracy the model was represented on the plate under the etching point. The idea of changing the model immediately suggested itself. The waved model was removed, and a shilling put in its place, when a tolerable copy was at once produced. A distinguished machine-engraver tried the same experiment by my directions, at his place in Somerstown near London, with great success. When I asked him how he had succeeded, his answer was, "perfectly, it would not only copy the waved model, but any device that I put under the touch."

During my stay in London, nothing was done to bring the art of medal ruling into notice. On my return to Philadelphia, I was the first to unveil the mystery of medal ruling, but being deeply engaged in introducing improvements in bank-note engraving, I gave no attention to it until about 1826 or '7, when thinking it might afford an additional security against counterfeiting, I put my machine a little in order for experimenting, and executed a large plate, containing medals, a copy of which I herewith send you. The work was much admired. Impressions from the plate soon found their way to Europe, and excited to exertion the ingenious mechanicians of London and Paris. Mr. Saxton of this city also exercised his ingenuity on the subject. I do not perceive, however, in these specimens a greater degree of perfection than in my own. I find not the least difficulty in believing, that Mr. Saxton was enabled, by his own scientific skill, to remove the cause of the distortion. When this defect was pointed out to me in my specimen, which was not till some time after it was done, (it was not very glaring, and, in low relief, would not have

been noticed,) I took an opportunity to search for the cause, and when found, it was, in my machine, remedied with great ease. I had only to lower the medal, allowing the touch to follow it down, until a line, drawn from the point of contact to the joint or fulcrum on which the touch turned, was at the proper angle of descent, and from the great number of productions which I have examined, there must be many besides Mr. Saxton and myself, who have got over the difficulty with the same ease.

I have been thus circumstantial, in order to shew how the result was arrived at. The circumstances which I have detailed will be recollected, in general, by all those who were in the establishment of Murray, Fairman & Co. at the time, but fully and particularly remembered by Mr. Gobrecht. All of this may have little to do, perhaps, with medal ruling in the abstract, the origin of which, in its truth and simplicity, may be made known in a few words. No one's ingenuity was taxed for this particular purpose. The movement taken from the Rose Engine, and by me applied to a ruling machine of my own invention, was designed for procuring waved lines and nothing more. When it was put in motion, it copied the wave with great truth and precision, and at the same time manifested, in a manner not to be mistaken, the fact, that it had also the power of copying medals with equal exactness and beauty.

In your account also it is said, that copies could not be taken *immediately* from the coins, because the picture would be reversed and the legends would read backwards, and that it was, therefore, necessary to obtain the impressions in metal hard enough to bear the tracer, and that without the seasonable invention of the electrotype by M. Jacobi, the work could not have gone on.

The modern art which you speak of, ingenious as it most certainly is and useful for many purposes, does not seem to me to be necessary, nor even called for in copying coins. The copies which I send you were taken sixteen years ago from the *original* medals. The devise may be reversed on the plate, simply by having the plate supported with its face *down* and the etching point pressed *up* to it, or by bringing the touch to act on the medal in the same way. Impressions of coins or medals, *if* necessary *at all*, are obtained with great facility in *shellac*, a material far preferable, in my opinion, to any metal whatever for such purpose, as it resists the tracer perfectly and causes no wear to its point.

I have thus addressed you on this subject, because I am interested in it, and because my name has been introduced into your work, in such a manner as to create the impression, that I was but a borrower from others of a machine, of which I was, in fact, the first and original inventor.

I am sure, gentlemen, that you will excuse the trouble which I give you in this communication. You are artists yourselves, and know, therefore, with what a jealous pride professional reputation ought to be guarded. I should be the last man to deprive another of any portion of his just desert, and I am equally unwilling that others should treat me with the like injustice.

Your obedient servant,

ASA SPENCER.

Philadelphia, July 26, 1842.

Sir—We have to acknowledge the receipt of your communication of the 21st inst., in which you object to the account we have given, in our "Manual of Coins and Bullion," of the origin and progress of the art of medal ruling, and complain that we "have introduced your name into our work, in such a manner as to create the impression that you were but the borrower from others of a machine, of which you were, in fact, the *first* and *original* inventor."

We can assure you that we feel great anxiety to do exact justice, as to the claims for an ingenious invention to which we owe so much, and that we would gladly take the earliest opportunity of correcting any error into which we might have fallen.

We cannot think, however, that you have ground for complaint, when we have before us the following original certificates; the first under your own signature, the second under that of a gentleman now an officer in one of the branch mints, and a man of unimpeached veracity. The first was published in the U. S. Gazette, and is as follows:

"I did not see the article in the U. S. Gazette of July 17th, signed Justice, concerning the invention of a ruling machine, before it appeared in print, consequently could not prevent the error contained in that article, which has since been pointed out to me. I take this opportunity to correct it, by saying, that it was never intended to deny, but always to admit, that Mr. Gobrecht was the

first to discover the mode of ruling medals, as exhibited by his specimen, published in 1817.

(Signed,) ASA SPENCER."

Philadelphia, Aug. 8, 1830.

Second certificate.—"I do hereby certify, that in the year 1816, being then employed by Messrs. Murray, Draper, Fairman & Co. in their establishment, with Mr. Gobrecht and Mr. Spencer, I was in the knowledge of the circumstance of Mr. Gobrecht's employing Mr. Spencer to make for him a ruling machine, the plan of which was, that the plate was to be moved under the ruler by means of a screw, and a machine was actually commenced to operate on this principle. The screw, however, was abandoned, and the machine was constructed so as to graduate with a roller, and merely to divide and rule straight lines. While this machine was constructing, Mr. Gobrecht explained to me a plan he had invented, to copy, by a ruling machine, medals, and surfaces slightly or deeply undulated, which he wished me to keep a secret, as it might be of great importance, and before June, 1817, he shewed me his machine, with the part attached to copy medals and surfaces, which was constructed by himself, and in successful operation. At the same time he shewed a head of Alexander, executed by means of the machine.

(Signed,) D. H. MASON, *Machinist and Engraver.*"

July 26, 1839.

The above statements establish all the claims of Mr. Gobrecht. You certainly made the ruling machine for him, and doubtless, with your well known skill and ingenuity; but it was without the appendage necessary to adapt it for medal ruling. Your alteration of Mr. Gobrecht's plan of moving the platform, appears to have possessed no advantages; at any rate, Mr. Saxton has adopted the screw in the last instrument of his construction.

It surprises us that you make light of Mr. Saxton's device for removing the *distortion* produced by the original machine-engraving; and that you seem to suppose that the means of overcoming this defect, are so obvious as to have occurred "to many besides Mr. S. and yourself." Now we think, on the contrary, that the principle employed by Mr. Saxton, is exceedingly ingenious and by no means obvious. Many sagacious persons who have seen it in operation, have not been able to understand the principle upon which it acts; and it is certain that the art of medal

ruling was long kept dormant and useless for the want of some plan for remedying the distortion. Mr. Saxton brought his improvement into successful action in 1829, and made no secret of it. The method you employ is essentially the same. Does it date as early?

In conclusion we freely declare, that the art of machine-engraving is under great obligation to you for bringing it before the public, both in Europe and America, and for applying it, with great skill and taste, to many important purposes. In any history of the invention, however brief, your name must necessarily have a place. It is introduced in our description; and although more might have been said of your agency, it would have imposed on us a necessity of dwelling upon the merits of many others, such as Terrel, Bate, Collas and Nolte, who have done much to bring this brilliant discovery into public notice and favor, which would have led us into a narrative diffuse and out of place.

Sensible of the polite and respectful manner of your letter, we have endeavored to reply in the same spirit, and now subscribe ourselves, Your faithful servants,

JACOB R. ECKFELDT,
WM. E. DU BOIS.

A. Spencer, Esq.

Gentlemen—I duly received your answer under date of the 26th July. I have been necessarily delayed in my reply.

I regret that I am compelled once more to call your attention to the subject of my former communication, which, however, I do with the less reluctance, as I am fully persuaded of the sincere desire which you express to do exact justice and correct any error into which you may have fallen.

What I complain of as unjust to myself is this, that in your work it is stated that you were indebted to Mr. Gobrecht for the art of medal ruling, and that the first specimen was executed by him, with a machine of his *own invention*, whereas, in fact, the specimen was by the machine which I invented and made for him, and by means of an appendage to it, suggested and furnished by myself.

I never pretended that my invention of *this* ruling machine, or the plan or principle on which it works, has more to do with rul-

ing medals, than any *other* ruling machine on any other principle; but as a passage in your communication seems to intimate something of that sort, I beg leave to make a few remarks on that subject.

Early in 1817, when I undertook the making of a ruling machine, the only means tried for moving one, were two modifications of the inclined plane, viz. the wedge and the screw. Only the former was then in use. Two machines constructed on this plan were in this city, and were supposed to be the only two ever made in this country. Mr. Richard Fairman, of our establishment, owned one of them, and its possession was thought to give him great advantages over other artists. A ruling machine in those days was considered an important acquisition, but to get up one on the wedge principle was such a formidable undertaking that few had the resolution to encounter it. It was an awkward and unwieldy contrivance, and though the wedges or inclined planes were six or eight feet in length, its range of work was only three or four inches, and the working of it was laborious, slow and tedious. I had therefore only the screw to look to. It is very desirable that a ruling machine for the ordinary work of the engraving office, should be so constructed, that various parts of the plate may be brought under the etching point with facility and despatch. I could hit upon no way of doing this with the screw, without more labor than I had to spare. I therefore gave it up, and took in its place the principle of the wheel and axle, which I thought admirably adapted to the purpose. All the essential parts were of simple forms, and required no extraordinary skill or implement to produce them; its movement was light and easy, the carriage could be shifted from one point to another without trouble or delay; its dimensions were not more than one third of those of the wedge machine, and its range was six times as great. These advantages were obvious and decisive. Mr. Fairman gave up his wedge machine, and lost no time in procuring one on my plan, and when seen in London, it met with the decided preference of the most eminent engravers of that city.

You say that the machine which I had made for Mr. Gobrecht, was without the appendage necessary to adapt it to medal ruling. I assert that Mr. G. had already been put in possession of this appendage by me, and at my suggestion, before the machine was

commenced, and that it was constructed with special regard to the adaptation of this appendage for waved line ruling. The slide with joints of peculiar construction was wanted only for that purpose, and was not known or used in any other ruling machine.

In your reply, two certificates are brought forward to show that no ground of complaint exists on my part—the first under my own signature, and the second, that of Mr. Mason.

In my communication I admitted—I always admitted, that Mr. Gobrecht was the first to discover the mode of ruling medals by a machine; or in other words, that he was the first to discover, that the appendage which I suggested and prepared for his machine, for ruling *waved* lines, was equally capable of ruling *medals*. I do not see that my certificate admits more than this, and nothing more was intended by it. Nothing more was due. It was drawn from me much against my will, during a newspaper discussion, which arose without my knowledge, advice or suggestion. I felt extreme reluctance to be dragged into public controversy, for which I lacked time, inclination and ability, and it would, moreover, have been very disagreeable to those with whom I was connected in business. And I was willing thus to terminate the dispute. The first part of Mr. Mason's certificate agrees with my statement. The difference which appears in the other parts is unessential, or is reconcilable therewith, without an imputation in the least degree unfavorable to any one. Mr. Mason himself is well known to me, and is justly respected by all who know him for his integrity and professional skill. The only discrepancy which I can perceive, is in that portion of his certificate where it is said, that "when the machine was *constructing*, Mr. Gobrecht explained to me a plan he had *invented* to copy, by a ruling machine, *medals*, and surfaces slightly or deeply undulated." The plan here spoken of is undoubtedly the one suggested and furnished by me to Mr. Gobrecht. Although Mr. G. was in possession of this plan before the machine was commenced, it is not easy to suppose that Mr. G. more than myself, anticipated *medal* ruling from it. The supposition is not unreasonable, nor altogether improbable, that when Mr. G. made his confidential disclosure to Mr. Mason, it being new to him, he supposed, of course, that it was Mr. Gobrecht's invention, and if at this time the machine was finished and in Mr. G.'s possession, he knew

that it would copy medals. These suppositions are rendered the more probable to me by the way in which Mr. Mason related the matter to me. He said Mr. Gobrecht invited him into his private room, showed him a machine of his invention for ruling medals, and exhibited as a specimen of its work a copy of the Alexander medal. Thus was conveyed to my mind the belief, that this was the first time that Mr. Mason had seen or heard of medal ruling.

It is remarkable that the Rose Engine, with one of its movements so identical with that of medal ruling, should have been so long in use without leading to that art. Perhaps a simple description of its action in the Rose Engine and in the ruling machine, may not be out of place here. The Rose Engine has two principal movements; one circular, the other lineal. The latter is adapted to ruling waved lines, which, in fact, it does in the Rose Engine. A waved surface of hard metal is fixed to the machine, a tracer is adjusted to the model, and a cutter to the plate that is to be engraved. The machine is put in motion, when a waved line is cut on the plate. The plate is then advanced a step, the motion is repeated and another line is cut. Thus a series of lines is continued until the work is done; and as the tracer passes always on the same line over the model, the lines on the plate are similar. To show more clearly the result of this movement in the Rose Engine and in the ruling machine, I have prepared two specimens, one from each.

I have taken for a model the word "Londini" in raised letters. The first series is after the manner of the Rose Engine, the plate alone being moved, the tracer passing always on the same line, which you may perceive is just where the right hand thin stroke of the N joins the thick one. The model is then fixed to the carriage, and made to move with the plate. The tracer is placed on the plane, a few lines above the letters; the ruling is commenced and carried on until the tracer comes on the same line on which it acted during the first series. The first series is made up of one minute section of the model, and, consequently, can give no indication of the device that may be on it. In the second series, where the model moves with the plate, a different section of the model is brought under the tracer at every line. A course of minute sequent sections is marked on the plate, and, these imbodied, show the device on the model.

Thus this movement, when it is attached to any ruling machine, and the model and plate move together, must *necessarily* show any one that it can rule medals.

I am truly sorry if any thing which I have said is construed to imply the least disrespect for the ingenious contrivance of Mr. Saxton, for removing the distortion. Certainly it was very remote from my intention to speak slightingly of him or his device. Such injustice would do little credit to my judgment or candor. No one acquainted with that gentleman's extraordinary skill, will be at all surprised at the ingenuity of his contrivance. All that I meant to say is, that I, being limited as to time and resources, was constrained to economy, and forced to use a different and a simpler method of remedying the difficulty. "Mr. Saxton brought his improvement into successful action in 1829, and made no secret of it." You say the method employed by me is essentially the same, and ask if it dates as early. I cannot say precisely at what time it was done, though I am quite certain it was sometime *after* that year, but long before I had any knowledge that Mr. Saxton had done any thing in that way. Although I had no idea *how* the thing was done, I had seen that it *was* done before I attempted it. I lay no claim to originality in removing distortion. Candor also compels me to confess how little I deserve your compliments for extending, improving and applying the art of medal ruling.

I have always said and always endeavored to maintain, that it *originated* with me, in consequence of my attaching the Rose Engine movement to the ruling machine, for the purpose of ruling *waved* lines; and that the result was, that the first three persons who put this appendage in action for ruling *waved* lines, obtained, *each*, something like a copy of a *medal* at the same time.

Here the affair rested. Nothing was done for the machine, to improve or enlarge its medal-ruling power, until I revived the art about the year 1827. The want of leisure limited the use of it to our own business of bank-note engraving. It was not long after this that my specimens made their appearance in London. Although my machine was known there in 1820, not enough of medal ruling had been seen to attract attention. But the appearance of these specimens soon excited the ingenuity of different artists, and about 1830, we find Bate of London, Collas of Paris, and Mr. Saxton, powerful competitors in this art, the importance of which was soon manifested in their various labors.

You will observe therefore, in conclusion, gentlemen, that I claim the invention as well as the construction of the machine which was furnished to Mr. Gobrecht in 1817, and also that I was the first to adapt the Rose Engine appendage to the ruling of waved lines by it, in which this art originated. I never claimed to be the first to use it or discover its use for copying medals. Indeed for two years after its construction, I was ignorant, in common with others, of the mode in which this was done, although I discovered it, as I have already stated in my first communication, immediately on using the machine myself.

I might then if I had not been too much immersed in my professional concerns, and too reluctant to engage in controversy, easily have established beyond contradiction, every fact which I have here asserted. I hope I have convinced you of the mistake into which you have fallen. It cannot seem strange that I should be mortified, if in such a standard work as yours, an error so unjust to me should remain uncorrected.

Your obedient servant,

Asa Spencer.

April, 1843.

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